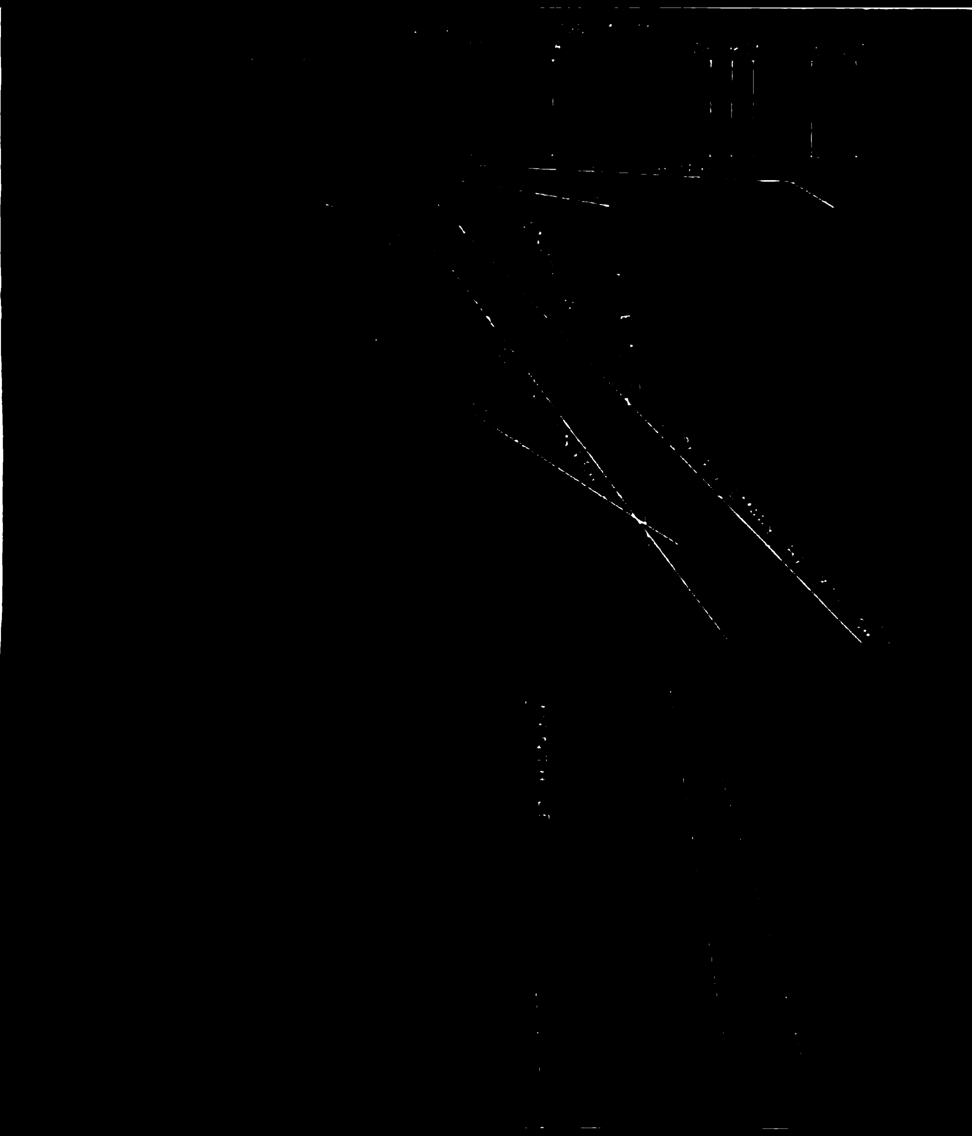

Proceedings of the Royal Society of London

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OF THE

ROYAL SOCIETY OF LONDON.

From November 18, 1880, *to March* 17, 1881.

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THE ROYAL SOCIETY.

"Researches on the Action of Organic Substances on the Ultra-Violet Rays of the Spectrum. Part III. An Examination of Essential Oils." By W. N. HARTLEY, F.R.S.E., &c., Professor of Chemistry in the Royal College of Science for Ireland, Dublin, and A. K. HUNTINGTON, F.C.S., Associate of the Royal School of Mines. Communicated by Professor STOKES, Sec. R.S. Received July 22. Read November 20, 1879.

Much chemical and physical research has been devoted to the class of bodies known as essential oils, as, for instance, the investigations of Dr. J. H. Gladstone ("Journal of the Chemical Society," vol. xviii, p. 1; vol. xxiii, p. 147; vol. xxv, p. 1), of Dr. C. R. A. Wright ("Isomeric Terpenes and their Derivatives," vol. xxvi, pp. 549 and 686; vol. xxvii, pp. 1, 317, and 619), and of Dr. W. A. Tilden (*loc. cit.*, vol. xxviii, pp. 514 and 1258), as well as many others.

It is now well known that essential oils consist for the most part of isomeric hydrocarbons, which may be divided into three polymeric groups, possessing the composition denoted by the formulæ—

$$C_{10}H_{16} \qquad C_{15}H_{24} \qquad C_{20}H_{32}.$$

To the first class belong the hydrocarbons derived from turpentine, orange, nutmeg, myrtle, and several others, while the second group includes the hydrocarbons from rosewood, cubebs, calamus, cascarilla, patchouli, and cloves. The third group is represented by colophene. Though of unknown constitution, these bodies exhibit a close relationship to benzene derivatives.

Many of the oils are articles of great commercial value and consequently are subject to adulteration, a fact which, in addition to the scientific interest they possess, led us to submit a number of specimens to examination in the spectroscope.

We have to acknowledge assistance rendered to us by the kindness of several gentlemen who supplied us with samples, namely, Dr. Glad-

stone, from whom we obtained many oils which he had previously examined, Mr. Farries, of the firm of Burgoyne, Burbidges, Cyriax, and Farries, Dr. Septimus Piesse, and Dr. W. A. Tilden, who has given us some remarkably pure preparations derived from turpentine and from Portugal essence. The diagrams which explain the optical characters of the various substances examined, and particulars concerning which here follow, were made in the same way as those appertaining to the Parts I and II of this research. The wood cuts made from our diagrams have had the light and dark parts reversed, so that they differ from the lithographs of the same character published in the Phil. Trans., Part I, 1879: they have of course been greatly reduced in dimensions, but not all to the same scale. The layer of liquid examined was 15 millims. instead of 19 millims. thick.

We have found that the following essential oils and hydrocarbons transmit continuous spectra:—

The four isomeric terpenes (Diagrams 1, 2, 3, and 4) prepared by Dr. Tilden—

Australene, from common turpentine obtained from the *Pinus Australis*. Boiling-point 156–157°, specific gravity at 0° C.=0·876, 100 millims. rotate the polarised ray of light 22° to the right.—Undiluted, this hydrocarbon transmits rays lying between the 12 and 17 Cd. lines. Solutions of the substance in alcohol were examined containing $\frac{1}{100}$, $\frac{1}{500}$, $\frac{1}{1000}$, and $\frac{1}{5000}$ of their volume of the terpene. No absorption band was seen.

Terebene,* prepared from American turpentine by precipitating with sulphuric acid. Boiling-point 156°, specific gravity not ascertained, optically inactive.—The spectrum of the undiluted substance was much the same as that of *australene*. Further examinations were made of solutions containing $\frac{1}{100}$, $\frac{1}{200}$, $\frac{1}{400}$, $\frac{1}{1000}$, $\frac{1}{5000}$ of the volume of the liquid. The spectrum was continuous.

Terebenthene, from French turpentine. Boiling-point 156–157°, specific gravity at 0° C.=0·876, 100 millims. rotate the polarised ray 27½° to the left.—The transmitted rays were much the same as before. Solutions containing $\frac{1}{50}$, $\frac{1}{100}$, $\frac{1}{500}$, $\frac{1}{1000}$, and $\frac{1}{5000}$ of their volume of the substance were examined.

Hesperidene, from Portugal essence. Boiling-point 176–177°, specific gravity at 0° C.=0·859, 100 millims. rotate the polarised ray 96° to the right.—Dilute solutions containing $\frac{1}{100}$, $\frac{1}{500}$, and $\frac{1}{5000}$ of their volume of the liquid were photographed.

The four diagrams drawn from the photographs obtained from these substances show the rays transmitted by 15 millims. of the liquid alone and when diluted; all rays to the right of the curves are those absorbed by the liquid, while those to the left are transmitted.

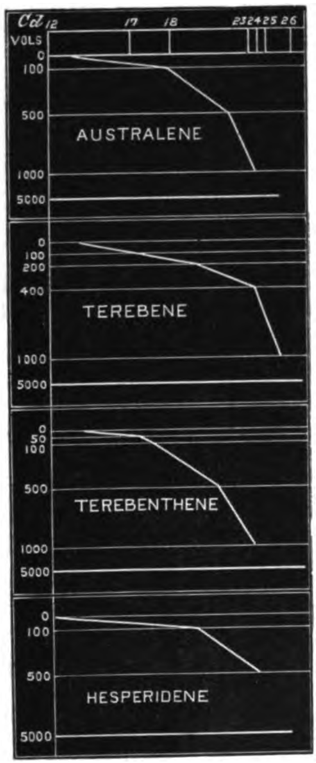
* This substance has been shown to be chiefly camphene. See "J. Chem. Soc.," Trans., Vol. 35, p. 758, 1879.—W. N. H., June, 1880.

The general similarity of the curves is a noticeable feature as well as the intensity of the absorption of the extremely refrangible rays.

DIAGRAMS 1—4.

Showing the rays transmitted after dilution with various proportions of alcohol.

Isomeric Terpenes.



The ordinates represent the proportions of alcoholic solution containing one volume of the terpene. Thickness of the layer of liquid = 15 millims.

It will be seen too that australene and terebenthene show slightly different curves, the latter being rather less diactinic when diluted 100 and 500 times. Another difference between these bodies is that one rotates the polarised ray to the right and the other turns it to the left.

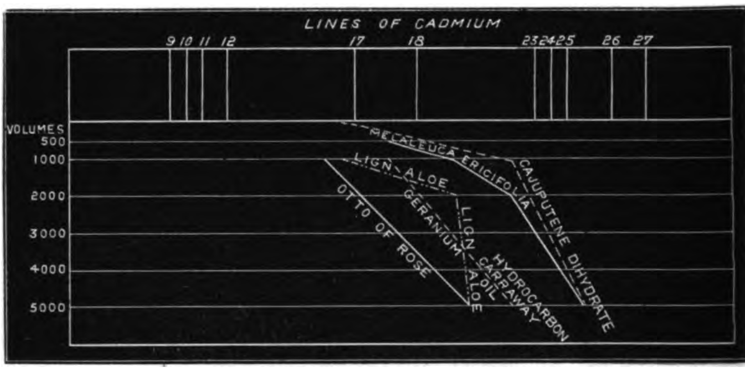
The following substances were from Dr. Gladstone's collection:—

Cajputene dihydrate.—The main portion of this liquid boiled at 173° C., and none distilled over below the temperature of 170°. Solutions containing $\frac{1}{500}$, $\frac{1}{1000}$, $\frac{1}{5000}$ were examined. An absorption,

caused, it was afterwards found, by a slight admixture of cymene was noticeable. (Diagram 5.)

DIAGRAM 5.

Oil of Indian geranium and two samples of the carraway hydrocarbon give the same spectrum.



Cajuputene dihydrate exhibits the cymene absorption band at $\frac{1}{500}$. The lign aloe spectrum lengthens out very considerably, but feebly, at $\frac{1}{2000}$.

The ordinates represent the proportions of alcoholic solution containing one volume of the oils. Thickness of the layer of liquid = 15 millims.

Oil of Lign Aloe.—This substance had no definite boiling-point, but distilled over between 185° and 200°, leaving a thick yellow resin in the retort. It was examined after diluting 1,000, 2,000, and 5,000 times. (Diagram 5.)

Carraway Hydrocarbon, No. 1.—This smelt like turpentine when distilled. The original label stated its specific gravity to be 0·8545. It boiled between 173° and 178°. The solutions examined contained $\frac{1}{1000}$ and $\frac{1}{5000}$ volume of the liquid. (Diagram 5.)

Oil of Indian Geranium, from Dr. Piesse.—This yielded the same result precisely as the carraway hydrocarbon. It was not re-distilled. (Diagram 5.)

Otto of Rose, from Mr. Farries.—The specimen was solid and crystallised in beautiful thin laminæ. It was not re-distilled. Solutions containing $\frac{1}{1000}$ and $\frac{1}{5000}$ of the volume of the melted substance were examined. (Diagram 5.)

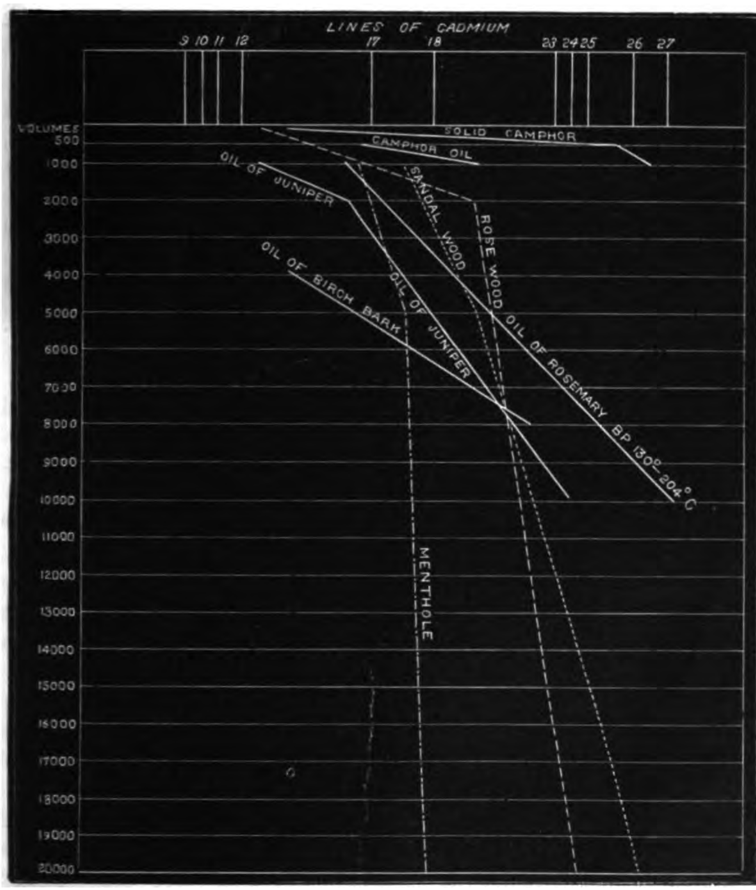
"*Santal Wood Oil.* Boiling-point 277°."* (Dr. Gladstone.)—The first fraction obtained on distillation was returned to the original bottle. The second part, which boiled between 277° and 287°, but chiefly between 277° and 280°, was the largest portion. The remainder distilled between 287° and 297°.

* Quotation from original label.

This substance withstands a large amount of dilution without great alteration in its strong absorptive power. It was examined when diluted 1,000, 5,000 and 20,000 times. (Diagram 6.)

DIAGRAM 6.

Carraway, cedrat, and santal wood oil give the same spectrum between $\frac{1}{1000}$ and $\frac{1}{5000}$.



The ordinates represent the extent of dilution of one volume of the various oils. Thickness of the layer of liquid = 15 millims.

Cedrat Oil (Dr. Gladstone).—This substance had no constant boiling-point; it distilled between 190° and 245°, yielding a yellow vapour and a thick yellow liquid, while a non-volatile yellow resin remained in the retort. Examined after dilution to 1,000 and 5,000 times its

original volume, it was found to possess exactly the same diactinic quality as santal wood oil. (Diagram 6.)

"*Carraway Hydrocarbon.* Boiling-point 350° F., specific gravity 0·8466."* (Dr. Gladstone.) No. 2.—The portion boiling above 173° C. was photographed. This substance, diluted to 1,000 and 5,000 times its volume with alcohol, yielded the same results exactly as the former sample and the oil of Indian geranium. (Diagram 6.)

Oil of Birch Bark (Dr. Piesse).—The specimen was not re-distilled. It showed a strong absorptive power until diluted to 4,000 times its own volume; from this point till an additional dilution of 8,000 volumes had been reached its diactinicity rapidly increased. (Diagram 6.)

Menthole. Boiling-point 225° C. (Dr. Gladstone.) Prepared from mint oil by means of the hydrosulphate.—After repeated distillations, during which water separated from the first portions, the greater part of the liquid boiled at 215° to 220°. This substance is remarkably adiactinic; even after diluting 20,000 times all rays from the line 18 Cd onward were absorbed. (Diagram 6.)

Oil of Juniper (Mr. Farries).—This was not re-distilled. When diluted 1,000 times, it transmitted very few of the rays beyond line Cd 12. When diluted 10,000 times it transmitted the spectrum as far as Cd 24. (Diagram 6.)

Oil of Rosemary (obtained from Italy by Dr. Gladstone).—It boiled between 180° and 200° C. When diluted 1,000 times it fails to transmit the rays near line Cd 17, but there is a steady increase in transparency on diluting 2,000, 4,000, and 10,000 times. (Diagram 6.)

"*Oil of Rosewood.* Boiling-point 480° F., specific gravity=0·9042."* —(Dr. Gladstone.) This is one of the polymerised terpenes, $C_{15}H_{24}$. It was found to boil at 250° C., yielding a pale yellow distillate. It increased rapidly in transparency on dilution from 50 to 2,000 times, after which stage up to 20,000 times the transparency did not greatly increase. (Diagram 6.)

Nutmeg Hydrocarbon.—This is the specimen specially referred to in Dr. Gladstone's paper. A few crystals resembling camphor were found in the bottle. The liquid was poured off from these. The first distilled fraction condensed in a turbid state and contained water. It boiled between 159° and 162°·5. The second fraction, equal to one-third the whole quantity of liquid, boiled between 162°·5 and 164°. The other three fractions boiled at temperatures ranging between 164° and 202°, and in addition to these portions there remained a solid residue.

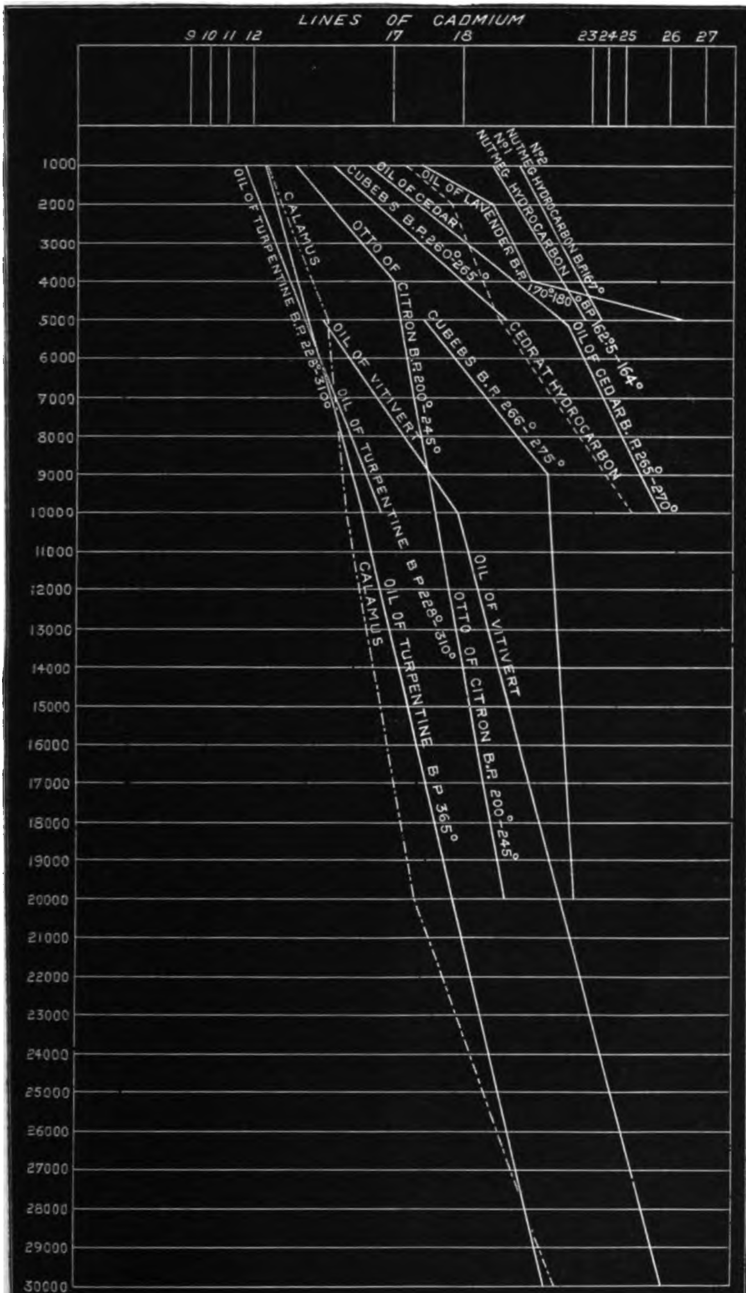
On dilution 1,000 and 5,000 times it proves to be amongst the most diactinic of such substances. (Diagram 7.)

Oil of Lavender (Mr. Farries).—It boiled between 170° and 180° C. This oil was very soluble in aqueous alcohol, which at once shows it to be perfectly free from turpentine. (Diagram 7.)

* Quotation from original label.

Diagram 7.—Calamus.—The spectrum beyond Cd 12 transmitted by a dilution of $\frac{1}{20000}$ was abnormally feeble.

Nutmeg hydrocarbons.—Specimens referred to in Dr. Gladstone's paper on "Essential Oils." No. 1, B.P. 162·5—164°. No. 2 contains a trace of cymene.



Dilution 1,000, 2,000, 4,000, and 5,000 times yields a series of solutions from which a remarkable curve may be traced, the lengthening of the spectrum being very rapid between the two last points.

Cedrat Hydrocarbon. (Dr. Gladstone.) Boiling-point 173° to 175°.—Dilution to 1,000, 2,000, 5,000, and 10,000 times yielded a curve somewhat similar to, but less striking than, that derived from the photographs of oil of lavender. (Diagram 7.)

Oil of Vitivert (Dr. Gladstone).—Two fractions of this substance were distilled off the specimen, which was small in quantity. The first part contained a little water, and the oil was yellow; the second portion was of a peculiar greenish tint, doubtless because of the presence of some blue oil common to camomile and patchouli being here in presence of a yellow oil.

A brown resinous mass was left in the retort. The fraction photographed boiled between 280° and 285°. This is one of those substances which still absorb some of the more refrangible rays even when diluted 30,000 times, while a dilution of 10,000 transmits no rays beyond the line 18 Cd. (Diagram 7.)

Oil of Turpentine. No. IV in Dr. Gladstone's paper, "Journal of the Chemical Society," vol. xviii, p. 18.—This substance had become greatly altered by keeping. Its original boiling-point was 160° C. It had oxidised and no doubt become polymerised, since many fractions with high boiling-points were distilled off.

The following are the boiling-points of different portions of the entire distillate:—

150—160°, contained water.
160—165°, " "
165—180°, " "
180—205°, " "
205—228°, slightly turbid.
228—310°, the greater portion boiled between 238° and 280°.
310—365°.

At 320° the liquid was greenish in colour, and at 365° a yellowish-green vapour was evolved. The oxidised products evidently split up by the action of heat with the production of water and a hydrocarbon. The fraction boiling between 228° and 310° was very much more soluble in alcohol containing 30 per cent. of water than that with a boiling-point of 160—165°.

The fraction boiling between 228° and 310° did not transmit ray 12 Cd when diluted 1,000 times, and the spectrum was cut off at line 17 Cd, after diluting with 10,000 volumes of alcohol.

The fraction boiling above 365° did not differ much from the preceding till a dilution with 10,000 volumes of alcohol was reached.

The spectrum, however, terminated a little beyond 18 Cd only after diluting 30,000 times. (Diagram 7.)

Oil of Cubebs (Mr. Farries).—There were two portions of this oil, one boiling at 260—265°, the other between 266—275°. The first was examined, when diluted 1,000 and 5,000 times; the second portion after diluting 5,000, 10,000, 20,000, and 50,000 times. As far as line Cd 17 the spectrum was freely transmitted, but a dilution of 10,000 times only feebly transmitted the more refrangible rays. (Diagram 7.)

Oil of Calamus (Dr. Gladstone).—This oil belongs to the group of substances with the formula $C_{15}H_{24}$. The portion boiling at 260° was photographed. It yielded spectra after diluting 1,000, 10,000, 20,000, and 30,000 times, and throughout exhibited the same absorptive power, or nearly so, as that displayed by the oil of turpentine of highest boiling-points. (Diagram 7.)

Otto of Citron (Dr. Gladstone).—On the second distillation, two fractions were separated: the first boiled between 110° and 200°, the distillate containing at first a little water; the thermometer then rose rapidly to 200°. The second fraction boiling between 200° and 245° was photographed. This is one of those substances exhibiting a strong absorption, even after dilution with alcohol to 20,000 times its original volume. (Diagram 7.)

Oil of Patchouli. No. 1. (Obtained by Dr. Gladstone from Mr. E. Atkinson.)—On distillation, the three principal fractions boiled at the following temperatures:—

1st. 250—259°; distillate turbid.
2nd. 259—275°.
3rd. 275—290°; this portion was blue.
4th. 290—360°; this appeared to yield a blue vapour.

The second portion was photographed after diluting 1,000 and 5,000 times. (Diagram 8.)

Patchouli. No. 2.—This blue oil was rectified, and the portion boiling between 275° and 277° was photographed after diluting 5,000, 8,000, 10,000, and 50,000 times.

A very distinct absorption band was noticed lying between lines 12 and 17 Cd. (See diagram.)

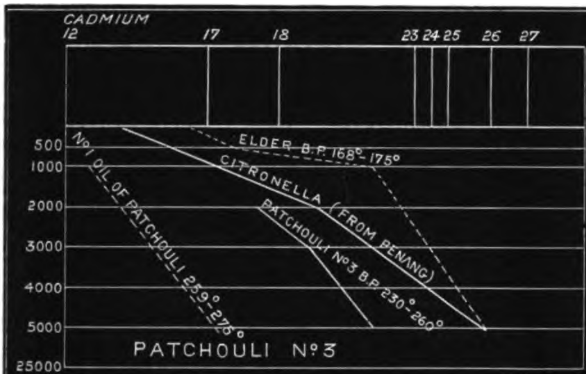
Perfect transparency to the more refrangible rays was not obtained by a dilution with 50,000 volumes of alcohol. This blue oil may be considered a benzene derivative, since it yields an absorption band between the lines 12 and 17 Cd., and is highly coloured.* (Diagram 9.)

Oil of Patchouli. No. 3.—Photographs of solution containing $\frac{1}{1000}$,

* All organic colouring matters of which the constitution is known are benzene derivatives in the sense that naphthalene and anthracene are benzene derivatives.—W. N. H.

$\frac{1}{3000}$, $\frac{1}{5000}$, and $\frac{1}{30000}$ of this oil were taken. It is remarkable for its strong absorption, though it shows no band. (Diagram 8.)

DIAGRAM 8.



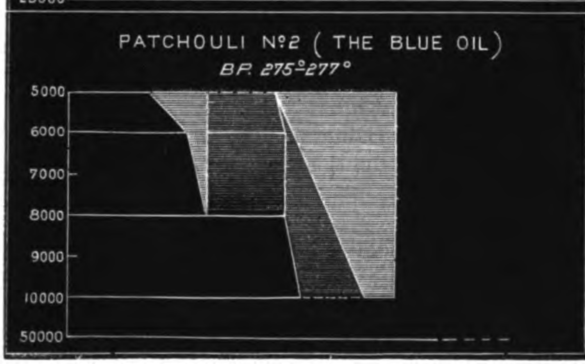


DIAGRAM 9.

Oil of Citronella. (From Penang. Dr. Gladstone.)—Two fractions were distilled off at the following temperatures:—

1st. 230—260°.
2nd. 260—300°.

The first portion was photographed undiluted, diluted 2,000 and 5,000 times. (Diagram 8.)

Oil of Elder (Dr. Gladstone).—This oil boiled between 168° and 175°. The oil itself and solutions containing $\frac{1}{500}$, $\frac{1}{1000}$, and $\frac{1}{5000}$, were examined. (Diagram 8.)

Oil of Melaleuca Ericifolia (Dr. Gladstone). Boiling-point 170—180°.—Faintly yellow. Photographs were taken after diluting to 500, 1,000, 2,000, and 5,000 volumes. (Diagram 5.)

Oil of Cedar Wood (Dr. Gladstone).—There was no distillate at any temperature below 250° C., and only a few drops at 260°.

1st fraction, boiling-point 260—265°.
2nd „ „ 265—270°.

This oil, like several others, polymerises easily during distillation; consequently, a freshly distilled specimen on redistillation leaves a resinous residue in the retort. The portion boiling between 265° and 270° was examined after diluting 1,000, 3,000, 6,000, and 10,000 times. (Diagram 7.)

An examination of the diagrams which contain the results of observations on the preceding substances will show that in bodies of the same constitution the absorption of the ultra-violet rays is greater the larger the number of carbon atoms in the molecule. That is to say, the oils with the higher boiling-points such as calamus, patchouli, and the denser turpentine oil, are those which are the least transparent after dilution with alcohol.

With two exceptions, none of the substances already examined exhibit absorption bands. The exceptions alluded to are the first specimen of carraway hydrocarbon, which we shall show further on, containing a small quantity of cymene, and the blue oil from patchouli.

As we shall have occasion to remark on the evident presence of cymene in several essential oils, it may be convenient to give an account of specimens of cymene which we have examined, and direct attention to the absorption bands which are made to appear by diluting the liquid to various degrees, as shown in the diagram.

Cymene No. 1.—This specimen was prepared by Dr. C. R. A. Wright, and examined optically by Dr. Gladstone. Its source was probably oil of lemons. It began to distil at 172°, and nearly all the liquid came over below 176° C.

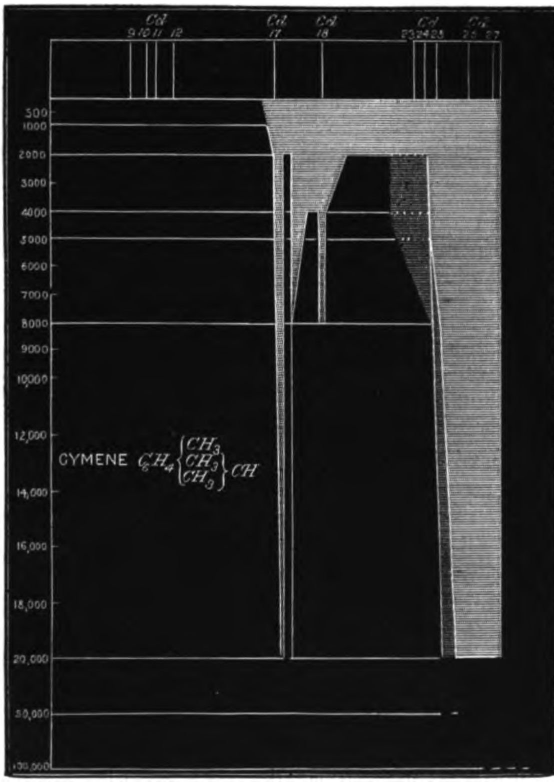
The portion boiling between 173° and 175° was completely soluble in fuming sulphuric acid with only a pale brownish coloration. It was examined first without dilution, and subsequently when diluted to 1,000, 2,000, 4,000, 5,000, 8,000, 20,000, 50,000 and 100,000 volumes. Three well-defined absorption bands are visible with a dilution of 4,000 and 5,000 times; a band similar to that lying below the line 17 Cd appears to be characteristic of the hydrocarbons derived from benzene. See diagrams of benzene, ethyl-benzene, mesitylene, toluene, &c., in Part II of this research (" Phil. Trans.," Part I, 1879). (Diagram 10.)

Cymene No. 2.—About 250 grms. were obtained from Mr. Kahlbaum's agents. The boiling-point of nearly the whole of it lay between 173° and 176°. No portion of this was so pure as the

preceding specimen. When diluted 2,000, 4,000, and 5,000 times it exhibited absorption bands coinciding with those shown in No. 1.

These bands are noticeable in the carraway hydrocarbon.

DIAGRAM 10.



The following specimens of essential oils and hydrocarbons prepared therefrom all exhibit absorption bands.

Thyme hydrocarbon.	Oil of bergamot.
Lemon ,,	Myristicol.
Nutmeg ,,	Oil of cloves.
Oil of bay.	Oil of aniseed.
Otto of pimento.	Carvol.
Oil of thyme.	Oil of cassia.
Oil of peppermint.	

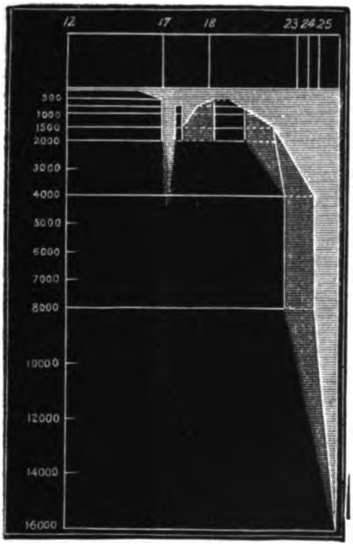
In the case of the hydrocarbon from thyme, lemon, and nutmeg, it is the presence of cymene which causes an intermediate absorption in

the spectrum. In other cases absorption bands are present because the oil itself is largely composed of some other benzene derivative.

Thyme Hydrocarbon. No. 2. (Dr. Gladstone.)—After careful distillation and fractioning, there were two portions which boiled at 161—163°, and 163—170°. A considerable quantity derived of course from the first fractions boiled between 162° and 163°, and was taken as the representative of the pure substance. The liquid diluted to 50, 500, 750, 1,000, 1,500, 2,000, 4,000, 8,000 and 16,000 times its original volume was examined. (Diagram 11.)

DIAGRAM 11.

Thyme hydrocarbon. (Dr. Gladstone.)



This absorption is due to the large amount of cymene contained in the liquid.

The curve shown is interesting, because it enables us to make an approximate estimation of the proportion of cymene contained in the hydrocarbon, for since the absorption exhibited when the dilution to $\frac{1}{750}$ coincides as nearly as possible with that given by cymene at $\frac{1}{4000}$, it is evident that the thyme hydrocarbon must contain about $\frac{1}{8}$ its volume of this liquid. The rounding off of the more refrangible portion of the spectrum is a modification caused by the terpene.

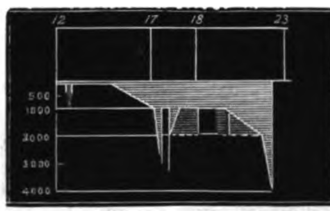
"*Hydrocarbon from Oil of Lemons.*—Boiling point 343° F., specific gravity 0·8468 at 20° C."* (Dr. Gladstone.)

* Quotation from original label.

Nearly the whole of this specimen boiled between 173° and 175° C. Photographs were taken after diluting 50, 1,000, 2,000 and 4,000 times. The cymene absorptions are seen at $\frac{1}{1000}$ and $\frac{1}{2000}$, and indicate that about $\frac{1}{7}$ of the volume of the original liquid consists of this body. (Diagram 12.)

DIAGRAM 12.

Hydrocarbon from oil of lemon. (Dr. Gladstone.)

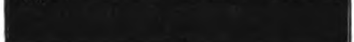


The absorption due to cymene is well seen in this specimen.

Hydrocarbon from Oil of Nutmeg.—Specimen No. 2. (Dr. Gladstone.) A very considerable quantity distilled at 167°, and as the boiling-point did not rise, a portion of this was taken for examination. Solutions containing $\frac{1}{100}$, $\frac{1}{1000}$, $\frac{1}{2000}$, and $\frac{1}{4000}$ of the liquid were examined. It showed the cymene absorption. (Diagram 13.) The

DIAGRAM 13.

Nutmeg hydrocarbon. B.P. 167° C. Absorption due to cymene.



diagrams of course vary a little in appearance according to the number of solutions photographed, and of course in these particular cases by the proportion of cymene present; hence the diagram representing the absorption in the case of the thyme hydrocarbon gives the best idea of the modification of the terpene spectrum caused by an admixture of cymene by reason of the greater number of photographs employed in depicting it.

Substances causing strong absorption bands in the spectrum transmitted by dilute solutions.

The following oils and derivatives of essential oils show strong absorption bands in their photographed spectra. For the most part they are bodies known to contain the aromatic nucleus as an essential part of their constitution. Thus the oils of bay, pimento, and cloves contain the substance eugenol, $C_6H_3.OH.OCH_3.C_3H_5$; oil of cassia consists of cinnamic aldehyde, $C_6H_5.C_2H_2.COH$; and oil of aniseed contains anethol, $C_6H_4.OCH_3\ C_3H_5$; and oil of thyme, thymol, $C_6H_3.OH_3.C_3H_7$.

Some other oils, such as bergamot and oil of peppermint, as likewise the bodies menthole, carvole, and myristicol, have an unknown constitution. The three latter substances are known to be isomeric. ("Journ. Chem. Soc.," Gladstone, vol. xxv, p. 1.)

Great interest is attached to our examination of these, since we consider it to be proved from the character of the spectra they transmit, that the nucleus of menthole is a terpene, while the benzene ring is the inner basis of carvole and myristicol. Bergamot appears to be a terpene mixed, with some derivative of the aromatic series, but oil of peppermint on the other hand is essentially a substance belonging to the latter class.

Oil of Bay (Dr. Gladstone).—There was a fair quantity of this substance, and the following fractions were separated by distillation:—

1st	fraction	boiling at	190—200° C.
2nd	,,	,,	200—220° ,,
3rd	,,	,,	220—240° ,,
4th	,,	,,	240—260° ,,

The two principal fractions boiling at 190 to 200°, and from 220 to 240°, were photographed.

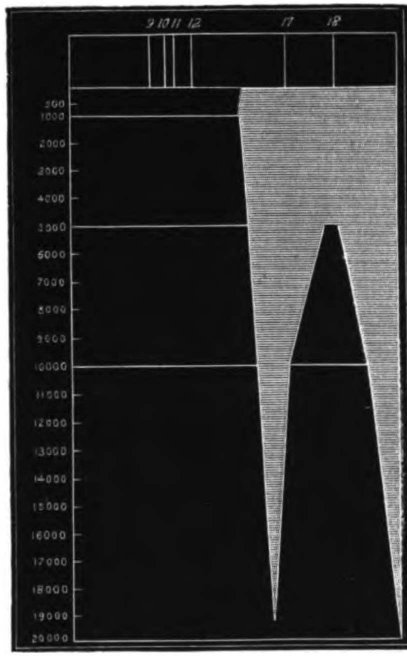
The fraction boiling between 190—200° solutions containing $\frac{1}{1000}$, $\frac{1}{5000}$, $\frac{1}{10000}$, and $\frac{1}{50000}$ were examined.

The first absorption band commenced midway between lines 12 and 17 Cd, and in a solution containing $\frac{1}{5000}$ of the oil, this continues nearly to the line 18 Cd; at this point a narrow band of rays is transmitted. The widening out of this band is somewhat rapid between $\frac{1}{5000}$, and $\frac{1}{10000}$, after which it is somewhat more gradual, there being still some absorption at 1 in 50,000.

The fraction boiling between 220—240° solutions containing 1 in 1,000, 5,000, and 10,000 were photographed. The absorption in these solutions appears at nearly the same points, but is more intense. This is quite what one might expect supposing the fraction with the higher boiling-point to contain a greater proportion of the absorbing body than the other.

Diagram 14.

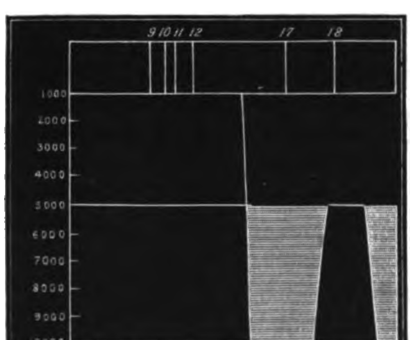
Oil of bay. (Dr. Gladstone.) Fraction boiling at 190—200°.



Absorption here is visible at $\frac{1}{80000}$.

Diagram 15.

Oil of bay. Fraction boiling at 220—240°.



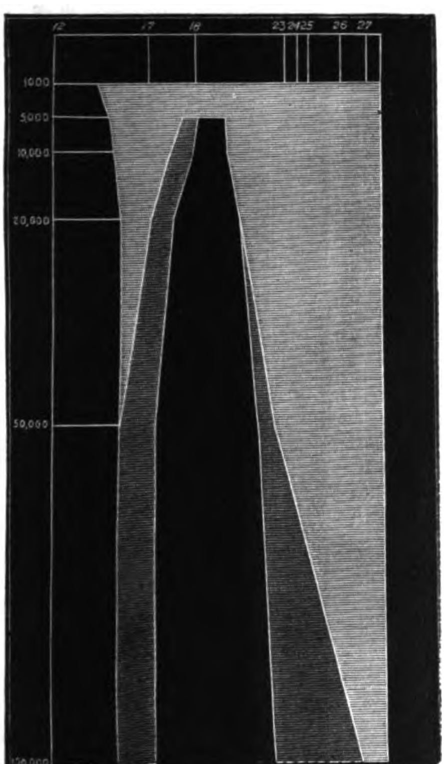
Absorption here is visible in a solution containing $\frac{1}{100000}$. It is evidently due to eugenol, $C_6H_3(OH)\left\{\begin{matrix}OCH_3 \\ C_3H_5\end{matrix}\right.$. Compare with oil of cloves.

There can be little doubt but that eugenol is the cause of the absorption in both cases.

Otto of Pimento (Dr. Gladstone).—The bulk of the liquid, consisting of some 5 or 6 fluid ounces of the oil, boiled between 238° and 241°. Eugenol has been shown by Bonastie to be contained in otto of pimento, and its boiling-point is said to be 242° (Stenhouse), 243° (Ettling), 248° (Brüning), and 251° (Greville Williams). (Gmelin, English edition, vol. xiv, p. 202.) We may consider, therefore, that the characteristic spectrum of this otto of pimento to be due to eugenol. Solutions containing $\frac{1}{1000}$, $\frac{1}{5000}$, $\frac{1}{10000}$, $\frac{1}{20000}$, $\frac{1}{50000}$, and $\frac{1}{100000}$ were photographed. (See Diagram 16.)

Diagram 16.

Otto of pimento. Absorption due to eugenic acid. See oil of cloves and of bay.



Oil of Cloves (Dr. Gladstone).—The following fractions were obtained on distillation:—

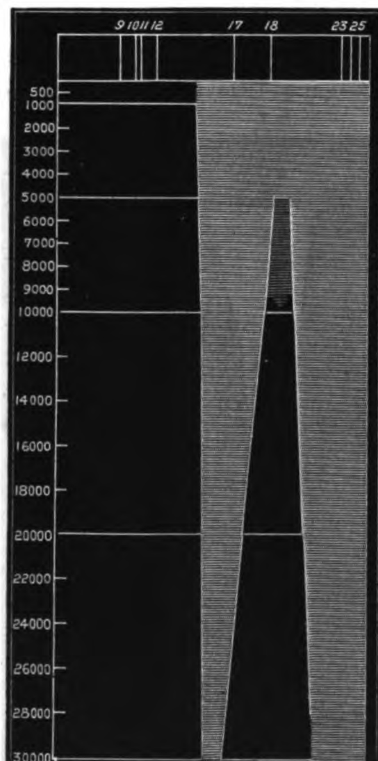
No. 1 boiled between 247—250°.
„ 2 „ 250—255°.
„ 3 „ 255—300°.
„ 4 boiled above 300°; a brown resinous residue was left in the retort.

The second fraction distilled almost entirely between 250° and 253°.

DIAGRAM 17.

Oil of cloves. (Dr. Gladstone's specimen.)

The absorption band is evidently due to eugenol, $C_6H_3(OH)\left\{\begin{matrix}OCH_3\\C_3H_5\end{matrix}\right.$.



Absorption still seen at 1 in 50,000.

Oil of cloves contains a small portion of a hydrocarbon, $C_{15}H_{24}$, together with eugenol (Ettling, "Ann. Pharm.," 9, 68, also Gmelin, English edition, vol. xiv, p. 202). The boiling-points of the hydrocarbons with the formula $C_{15}H_{24}$ lie between 249° and 260°. It is therefore extremely improbable that the hydrocarbon could be sepa-

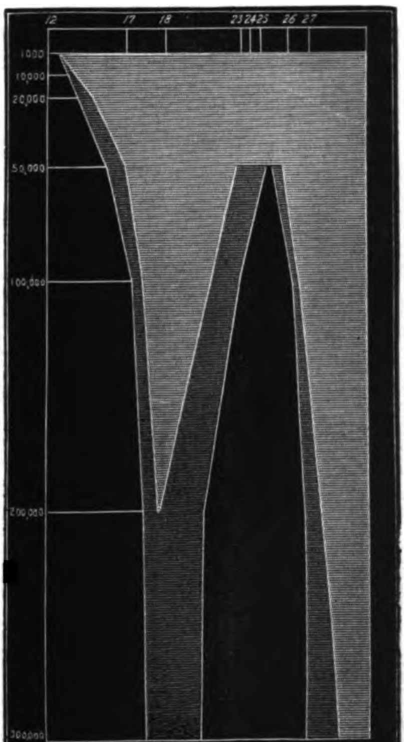
rated from the eugenol by distillation. As we desired to examine the oil of cloves in its usual state, we did not attempt to separate the eugenol by means of alkalies. That portion boiling between 250° and 253° was examined after diluting 1,000, 5,000, 10,000, 20,000, and 30,000 times. A characteristic absorption, similar to that of otto of pimento, was remarked. (Diagram 17.)

Oil of Aniseed (Dr. Gladstone).—By far the greater portion of this oil distilled readily between 220° and 223°, and was very easily crystal-

DIAGRAM 18.

Oil of aniseed. B.P. 220—223° C. The absorption band is due to anethol,

$$C_6H_4\left\{\begin{matrix}OCH_3\\C_3H_5\end{matrix}\right. .$$



Absorption still strong at 300,000.

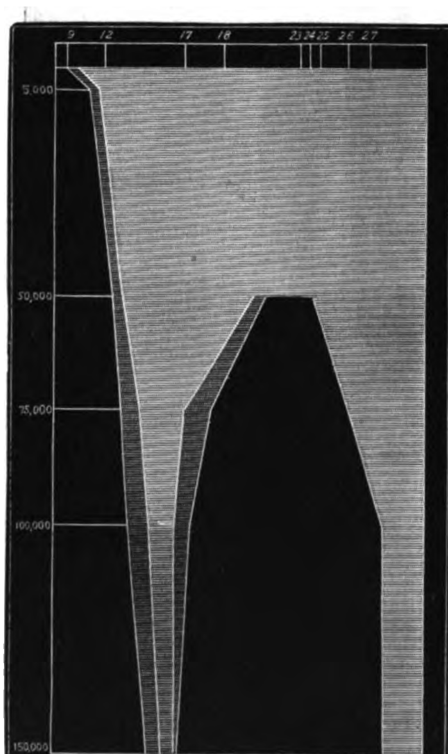
lised at a little below 12°. A portion, boiling between 220° and 223°, was examined. Photographs were taken of solutions containing $\frac{1}{1000}$, $\frac{1}{10000}$, $\frac{1}{20000}$, $\frac{1}{50000}$, $\frac{1}{100000}$, $\frac{1}{200000}$, $\frac{1}{300000}$ of the oil. The intensity

of the absorption exerted by this substance is extraordinary, and we may no doubt regard it as being very nearly pure anethol or allyl-phenol-methyl ether. (See Diagram 18.)

Oil of Cassia (Dr. Gladstone).—This specimen began to distil at 130° C., but the thermometer rose at once to 250° and then slowly continued upwards to 280°, the distillate being bright yellow. The remaining portion was a brown resin. The portion boiling between 250° and 280° was dissolved in 1,000, 5,000, 50,000, 75,000, 100,000, and 150,000 parts of alcohol, and examined. (See Diagram 19.)

Diagram 19.

Oil of cassia. B.P. 250—280°. The absorption band is due to cinnamic aldehyde, C_9H_7OH.



Absorption band very faint at 150,000.

The absorption is remarkably intense up to the dilution of 1 in 50,000, when a band of rays is transmitted adjacent to the position of

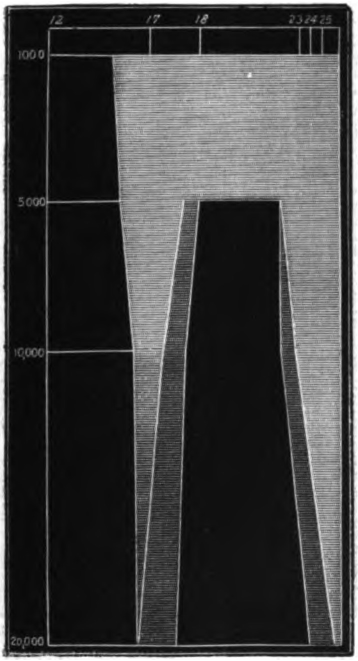
the line Cd 23. The absorption disappears as this band widens out by reason of further dilution.

Since oil of cassia consists mostly of cinnamic aldehyde, which has the composition $C_6H_5.C_2H_2.COH$, the aromatic nucleus is here again accountable for the absorption.

Oil of Thyme (Dr. Gladstone).—Nearly the whole of the specimen distilled between the temperature of 220° and 240° C. A brown resinous residue remained in the retort. Portions of the oil were examined in solutions containing $\frac{1}{1000}$, $\frac{1}{5000}$, $\frac{1}{10000}$, and $\frac{1}{20000}$. (See Diagram 20.)

DIAGRAM 20.

Oil of thyme. Portion boiling 220—240°. Absorption due to thymol.



A characteristic absorption may be traced to the presence of thymol, a substance already examined with other benzene derivatives. (See Part II of this research.)

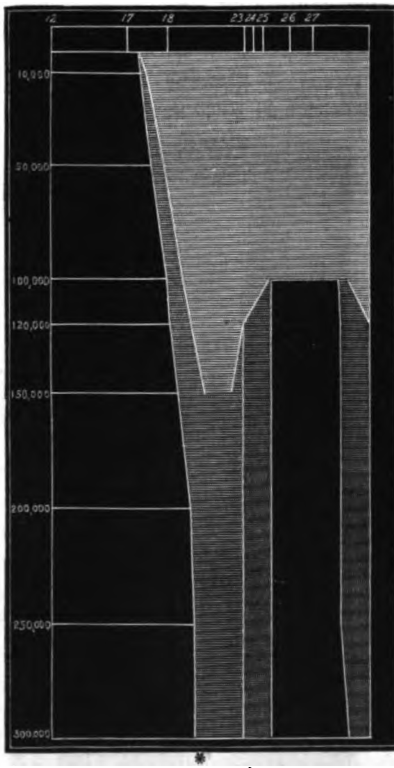
Carvol (Dr. Gladstone). Separated from oil of carraway by distillation.—The liquid distilled almost entirely between 215° and 220°. The portion boiling between these temperatures was examined after dilution 1,000, 10,000, 100,000, 120,000, 150,000, 200,000, 250,000, and 300,000 times its volume with alcohol. (See Diagram 21.)

The absorptive power of this body is remarkable, and is undoubtedly

due to its containing a nucleus of three doubly-linked carbon atoms. Its refraction equivalent is abnormal, like those of bodies of the aromatic series. (Gladstone, "Chem. Soc. J.," vol. xxiii, p. 149.) Furthermore it is isomeric with cuminic alcohol and thymol.

Diagram 21.

Carvol. B.P. 215—220°. $C_{10}H_{14}O$. Separated from oil of carraway by distillation.



* The absorption still very considerable.

These specimens were from Dr. Gladstone's collection.

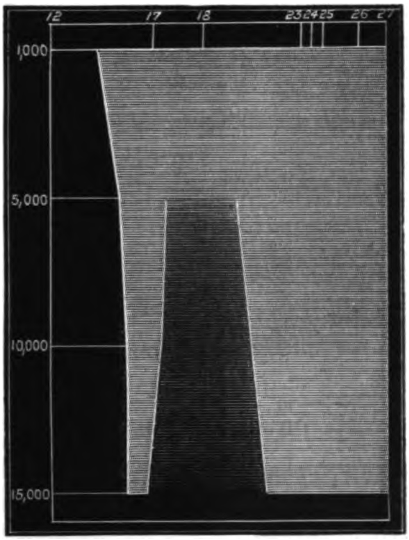
Myristicol (Dr. Gladstone).—Derived from oil of nutmegs. On this substance being distilled, there were two principal fractions:

No. 1, boiling below 215° C.
No. 2, „ from 215° to 260°.

The second portion was the better and much the larger part of the liquid. The liquid diluted 1,000, 5,000, 10,000, and 15,000 times, was photographed. The character of the absorption is such as to

make it probable that this is a mixture of a terpene with a large amount of some benzene derivative.

DIAGRAM 22.
Myristicol.



When a mixture of two substances exhibits absorption bands which are due to only one of them, the following characters are generally noticeable:—1st. There is not unfrequently a haziness about the transmitted rays; 2nd. The absorption bands are not well defined; 3rd. A comparatively limited amount of dilution suffices to obliterate the chief features of the absorption spectrum. All these points may be to some extent observed in the spectra of this myristicol, but the bands are yet so strong that they evidently belong to the predominant compound.

On referring to the refractive equivalent of myristicol (Gladstone, Chemical Society's Journal, vol. xxiii, p. 149), we find that it corresponds well with numbers characteristic of compounds of the aromatic series. Hence, the conclusion is obvious, that the greater part of the liquid consists of some benzene derivative.

Oil of Bergamot (Dr. Gladstone.)—This oil was separated into two fractions:—1st, boiling between 175° and 180°; 2nd, between 180° and 190°.

The first portion was diluted to 400, 800, and 1,000 times its own volume, and photographed. An absorption band was noticed, which soon became removed by dilution, and therefore was set down to an impurity. (See Diagram 23.)

DIAGRAM 23.

Bergamot.

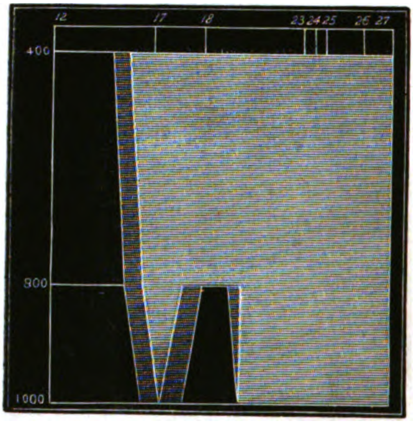
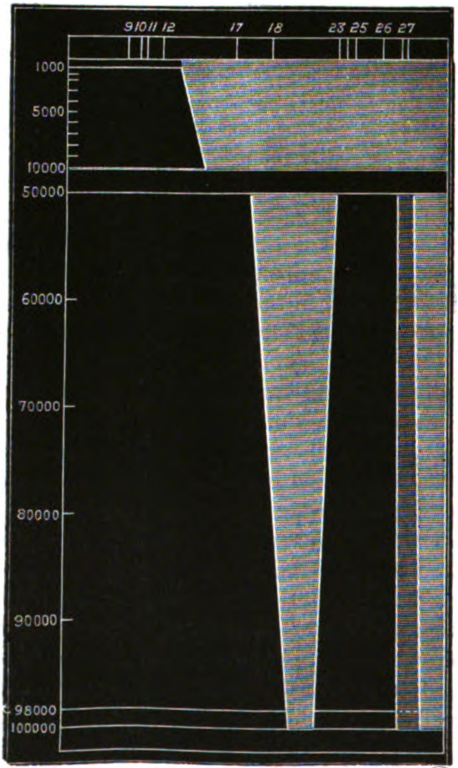


DIAGRAM 24.

Oil of bitter almonds.

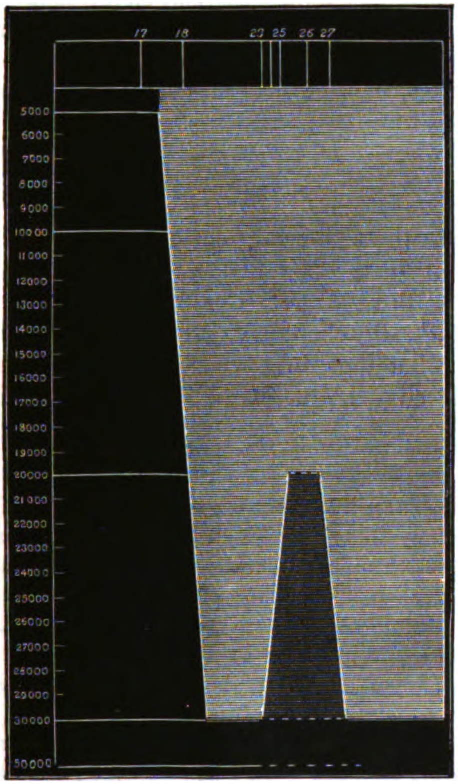


Oil of Bitter Almonds (Dr. Piesse).—Solutions in 1,000, 5,000, 10,000, 50,000, and 100,000 volumes of alcohol were examined.

A highly characteristic absorption distinguishes this substance. As is well known, it is the aldehyde of benzoic acid. (See Diagram 24.)

DIAGRAM 25.

Oil of peppermint. B.P. 198—215°. (Dr. Gladstone.)



Oil of Peppermint (Dr. Gladstone). B.P. 198—215°.—This is obtained by distilling *Mentha piperita*. Dr. Gladstone states that both the English and Italian specimens examined by him contained a hydrocarbon which has physical properties differing little from those of the hydrocarbon from bay. It will be seen, however, that the absorption spectra due to these two substances are very different.

The following is a summary of our conclusions with regard to the terpenes :—

1. Terpenes, with the composition $C_{10}H_{16}$, possess in a high degree

the power of absorbing the ultra-violet rays of the spectrum, though they are inferior in this respect to benzene and its derivatives.

2. Terpenes, with composition $C_{15}H_{24}$, have a greatly increased absorptive power.

3. Neither the terpenes themselves nor their oxides nor hydrates, exhibit absorption bands under any circumstances when pure, but always transmit continuous spectra.

4. Isomeric terpenes transmit spectra which generally differ from one another in length, or show variations on dilution.

5. The process of diluting with alcohol enables the presence of bodies of the aromatic series to be detected in essential oils; and even in some cases the amount of these substances present may be approximately determined.

Researches into the Colouring-matters of Human Urine, with an Account of the Separation of Urobilin." By CHAS. A. MACMUNN, B.A., M.D. Communicated by A. GAMGEE, M.D., F.R.S., Brackenbury Professor of Practical Physiology and Histology in Owens College, Manchester. Received March 6, 1880. Read March 18, 1880.

I do not propose to discuss in this paper all the pigments which have been said to occur in urine, as their consideration would extend over a considerable space; and I shall, therefore, limit my observations to those which I have myself studied, and which are discoverable by means of spectroscopic observation. Notwithstanding the efforts of physiological chemists, at home and abroad, no one has been hitherto able to isolate the pigment known as urobilin. After many unsuccessful attempts, I have at length succeeded in isolating a pigment, which, on account of its spectroscopic and chemical reactions, appears to be urobilin in a pure state.

Although it has not been obtained in sufficient quantity to allow of a formula being assigned to it, I believe that I shall soon be able to obtain enough for this purpose. A preliminary examination has shown that it contains carbon, hydrogen, oxygen, and nitrogen.

Preliminary Remarks.—If nitric acid be added to a solution of bile before the slit of the spectroscope, the solution at once undergoes a change of colour, becoming green, blue, violet, red, and, lastly, yellow or brownish-yellow, and the spectrum is characterised by having two bands: a broad shadowy band, composed of two in orange and yellow, and a black band at Fraunhofer's line F. In a short time the shading in orange and yellow begins to fade, and at the time the oxidation process is completed, and the colour of the solution

has become yellow, nothing but the band at F is left. Jaffé ("Zeitsch. f. Chem.," v, 666) succeeded in isolating the pigment which gives the feeble bands on each side of D, and also that which gives the band at F. The pigment which gave this last band, when isolated, was brown-red in colour, soluble in alcohol, ether, and chloroform; the solutions

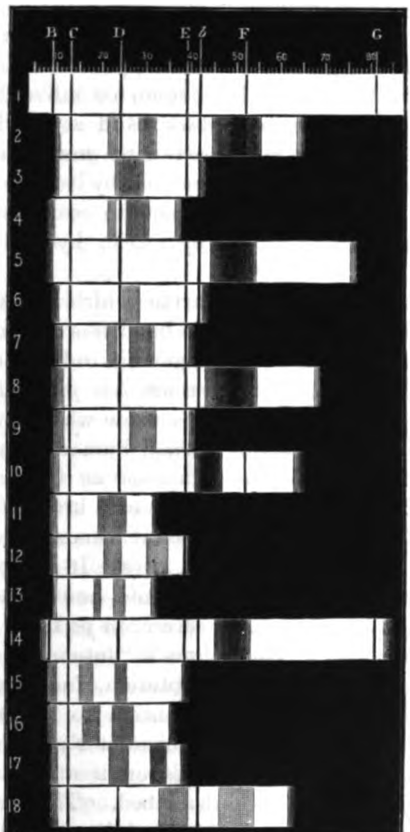


Figure reduced from Chart I. (See p. 36.)

being a fine red colour, giving, when acidulated, a dark band at F. By treating a solution of dog's bile with hydrochloric acid, the same observer obtained a red fluid, which became yellow on the addition of alkalies, and which, before alkalies were added, was marked by giving the same dark band as before, but after the addition of alkalies, especially caustic soda, this band was replaced by another one nearer the red. Since Jaffé described these appearances, Maly* has asserted

* "Ann. Ch. Pharm.," clxi, p. 368; clxiii, p. 77.

that he can produce urobilin, or, as he called it, hydrobilirubin from bilirubin, the red colouring matter of human bile;* and, although his researches have been called in question by some physiological chemists who profess to have repeated his experiments with a negative result, yet the conclusions drawn by him are practically correct, and have led, in my hands, to the discovery of urobilin in the bile of various animals, in human urine, and to its complete isolation from the latter fluid.

Most specimens of high-coloured urine, provided the high colour is not due to blood or unchanged bilirubin or biliverdin, show, when examined with the spectroscope, a dark band at F, which disappears completely when the urine is treated with ammonia, and which is moved towards the red end of the spectrum by treatment with solution of caustic soda. The band can be made to reappear after the ammonia treatment by the addition of nitric, hydrochloric, or acetic acid.

Since almost all specimens of urine which I examined showed a band at F, I had concluded that this band was due to urobilin, but I could not account for the fact that ammonia did not always cause its disappearance. I now find that there are *two pigments in urine which give a band at F.* This discovery was made while I was engaged in studying urobilin, for, on treating normal human urine, which was of a pale straw colour, in the same manner as that adopted for the separation of urobilin, I found a pigment of a brownish colour, which, when dissolved in ether, gave two faint bands, one of which was placed over F, the other between *b* and D (see 18 of figure). The band at F was not made to disappear by ammonia, nor did acids intensify it. As a similar band is found in blood serum, in yelks of eggs, in butter, cheese, &c., which is due to Thudichum's "lutein," I see no objection in accepting that author's name, urolutein, for this pigment. My object in mentioning this is to call attention to the fact that urobilin is not the *only* pigment which gives a band at F; but urobilin appears to be the only pigment which behaves on treatment with acids and alkalies in the manner I have described. The presence of this pigment urolutein in urine containing urobilin as well, has also led to the statement which appears in some text-books of physiological chemistry, that caustic soda, when added to urine containing urobilin, causes *two* bands to appear. The reason is, that caustic soda moves the urobilin band towards the red, but leaves untouched the band of urolutein; consequently, two bands are seen instead of one. When urobilin alone is present, ammonia causes the complete disappearance of the band at F, and caustic soda moves it towards the red. Accordingly, a preliminary examination will enable us to determine whether

* By reduction with sodium amalgam and subsequent treatment with hydrochloric acid.

urobilin alone is present, and the neglect of this observation has led to disappointment.

Examination of the Bile-Spectra of various Animals.—If urobilin be formed from bile, the question naturally arises, is urobilin present as such in the bile of any animal? If so, its biliary origin is made more certain. To enable me to reply to this question, I proceeded to examine this fluid in various animals, using in each instance fresh bile, and examining the spectra by means of a Sorby-Browning micro-spectroscope, and checking observations—when the amount of fluid at my disposal was sufficient for the purpose—by means of a one-prism chemical spectroscope.

The bile of the following animals was examined :—man, pig, dog, cat, guinea-pig, rabbit, mouse, sheep, hedge-hog, ox, crow, blackbird, chicken, goose, wild duck, duck, frog.

Among these animals, the bile of the following gave a characteristic spectrum :—guinea-pig, rabbit, mouse, sheep, ox, crow.

The darkest green or golden-red bile gave the least characteristic spectrum. I have fully described, in the "Spectroscope in Medicine," the colour and spectrum of each specimen, but I shall here merely mention those facts which throw light upon the origin of urobilin, namely, that by careful dilution or by examination in a sufficiently thin depth, a band at F is always visible, that this band is made darker by acids, and is made to disappear by adding ammonia. This is very striking in the case of the bile of the mouse (Chart II, sp. 12*), which gives a black band, resembling exactly that seen in febrile human urine. If Chart II be inspected, the most noticeable appearance is the presence of this band in so many spectra. And one cannot help also noticing the general resemblance between these bile-spectra and those shown in the figure, which are the spectra of urobilin in various solutions, and treated by various reagents.

I also attempted to extract urobilin from the liver of the pig, by means of various solvents, the pounded liver having been extracted with water, alcohol, ether, chloroform, and acidulated alcohol respectively, but without success.

Separation of Urobilin from Human Urine.—Before describing the method which I adopted for the separation of urobilin, I may make one or two preliminary observations. The reagents used were perfectly pure, and pure ethyl alcohol was used in every instance; this is a matter of great importance, as methylated spirit is not suitable for the separation of such easily decomposed bodies as urobilin.

The readings of the spectra are those of a photographed scale adapted to the micro-spectroscope; and before taking the readings, the precaution was always adopted of narrowing the slit until the sodium

* The Chart II referred to above is reproduced in the "Spectroscope in Medicine;" and accordingly does not accompany this paper.

line stood at the same number on the scale. Of course this would have been unnecessary if the jaws of the slit had been made to open equally.

The readings are also given in wave-lengths, the latter having been calculated by means of an interpolation curve; they are expressed in millionths of a millimetre.

Since preliminary observation had shown that there are two pigments in urine which give a band at F, it was necessary, if possible, to get a specimen of urine which contained urobilin only. Accordingly, the urine of a case of phthisis was chosen, which gave the band at F, in very slight depths; this band could be made to disappear by ammonia, and was moved towards the red by caustic soda. The colour of the urine was orange-red. The urine therefore contained urobilin, and on six different occasions I separated urobilin by the following method from it; on each occasion the result was the same. I may mention that no play of colours was produced by nitric acid (containing nitrous.)

Experiments.—230 cub. centims. of urine was precipitated by neutral lead acetate, and filtered, the filtrate which still showed the urobilin band was precipitated with basic lead acetate and filtered; the filtrate now showed no band. The precipitates were united and extracted with alcohol acidulated with sulphuric acid, and again thrown on a filter. The filtrate was a fine, clear red fluid giving in slight depths a well-marked black band at F. Some of this fluid was put into a separating funnel, a large quantity of water added, and then chloroform; the whole well shaken and allowed to stand, the red chloroform layer separated off and examined, when it was seen to give the same black band. The remainder of the alcoholic fluid was treated in the same manner. I had now a fine red fluid, being a solution of urobilin in chloroform, with, however, a little turbidity at its surface, which disappeared after filtration, which was twice repeated.

An attempt was made to separate some of the colouring matter from the chloroform with acidulated water, but with a negative result.

The chloroform was now distilled off, the residue redissolved in chloroform, which was again distilled off (in both cases over a water-bath). The residue was brown-red in colour, glistening on the surface, perfectly amorphous, and gave, when examined on the stage of the micro-spectroscope, with a strong light condensed upon its surface, a black band at F. It is perfectly soluble in alcohol, chloroform, nitric acid, hydrochloric acid, acetic acid, lactic acid, acidulated water, partially in ether, both ethereal solution and residue when dissolved in alcohol, giving same spectrum; partially in benzol and in water; insoluble in bisulphide of carbon.

The chloroformic solution gives no precipitate with chloride of barium, nor after treatment with ammonia.

Ethereal Solution of Urobilin, got by Sulphuric Acid and Alcohol Method described above.—The colour is dark red.

Spectrum—

Extent, 10 to 65 (or wave-length 678—457).
Band ϵ, 21 to 23 (or wave-length 604—592.)
Band δ, 28 to 32 (or wave-length 568—552).
Band α, 45 to 55·5 (or wave-length 507—479)
} See figure p. 27 (2).

By treating an appropriate depth of this fluid in a cell with ammonia, the colour becomes lighter and more yellow, and gives the spectrum shown in figure (3). At a less depth, the band at F is seen to have disappeared. This band shown in the figure (2) reads from 23 to 29 (or wave-length 592—564).

When excess of ammonia was removed by means of a small pipette, and the solution was acidified with acetic acid, the band at F reappeared.

Alcoholic Solution of same Pigment.—This solution was of a red colour, and gave, when examined at a suitable depth, two bands, and at a less depth another black band at F.

The readings of the bands at the first depth were as follows:—

Extent, 9 to 37 (or wave-length 686—534)
Band ϵ, 21 to 23 (or wave-length 604—592)
Band δ, 26 to 29 (or wave-length 578—564)
} See figure (4).

Less depth:—

Extent, 9 to 78 (or wave-length 686—435)
Band α, 45 to 55 (or wave-length 507—480)
} See figure (5).

When the first depth was treated with ammonia, a band appeared reading 25—29 (or wave-length 582—564), and in a slight depth of fluid so treated the band at F had disappeared.

This solution then gives the same spectrum as the ethereal solution, the slight differences in positions of bands being due to different refractive power of solvent used, and when ammonia was added, the same effect as in the case of the ethereal solution was produced. See figure (6.)

Chloroformic Solution.—Red solution, giving in sufficient depth two bands, and in less depth, one band.

The readings of the spectrum of first depth were as follows:—

Extent, 11 to 40 (or wave-length 671—523)
Band ϵ, 21 to 23·5 (or wave-length 604—590)
Band δ, 27 to 31 (or wave-length 573—556)
} See figure (7).

The band of the shallower depth read from 44 to 55 (or wave-length 510—480). See figure (8).

Treated with ammonia, I got the band shown in figure (9), reading

from 28—32 (or wave-length 568—552). So that this solution gives practically the same spectrum as the ethereal and the alcoholic.

Benzolic Solution.—Reddish solution giving following spectrum :—

Extent, 11 to 65 (or wave-length 671—457).
Band ϵ, 21 to 23 (or wave-length 604—592).
Band δ, 28 to 32 (or wave-length 568—552).
Band α, 45 to 55 (or wave-length 507—480).

This also gave a band near D when treated with ammonia, which also caused the disappearance of band α.

At first it seemed possible that the appearance of these bands ϵ and δ might have been due to the action of heat on the pigment during the evaporation, but on evaporating the chloroformic solution under the receiver of the air-pump the same result was arrived at.

Action of Caustic Soda on Solutions.—If the solutions were examined in a depth sufficient to show band at F distinctly and caustic soda were added, in each case this band disappeared and was replaced by another nearer the red. Thus in ethereal solution the band read 42—47 (or wave-length 517—502); and in chloroformic solution the band read 43—48 (or wave-length 513—499); and in alcoholic solution 41·5—47 (or wave-length 519—502.

Aqueous Solution.—The aqueous solution gave the black band at F, disappearing with ammonia and moving towards the red with caustic soda.

Solution of Pigment in Hydrochloric Acid.—It dissolved completely in hydrochloric acid, giving a red solution, which in a sufficient depth showed a band at D, figure (11).

Extent, 9 to 32 (or wave-length 686—552).
Band γ, 20 to 25 (or wave-length 609—582).

In less depth :—

Extent, 9 to 63 (or wave-length 686—462).
Band α, 45 to 53 (or wave-length 507—485).

By the ammonia and caustic soda treatment, the spectra were altered in the same manner as those of the other solutions.

Solution of Pigment in Sulphuric Acid.—A red solution.

Deep layer :—

Extent, 8 to 37 (or wave-length 694—534).
Band, 21 to 25 (or wave-length 604—582).

Shallower depth :—

Extent, 7 to 80 (or wave-length 702—432).
Band, 43 to 52 (or wave-length 513—488).

As all these solutions contained the same pigment, the next step

was to determine the elementary composition of this pigment; accordingly the residue from the chloroformic treatment was carefully analysed, when it was found to contain:—

Carbon.
Hydrogen.
Oxygen.
Nitrogen.
Sulphur.

The occurrence of the last element led to the supposition that it must have been due to the treatment with sulphuric acid. I therefore came to the conclusion that if this supposition were correct there would be an absence of sulphur in the pigment prepared by a method in which hydrochloric acid would be used instead of sulphuric.

Separation of Urobilin by means of Hydrochloric Acid.—This method was exactly similar to the first, with the exception, that instead of using alcohol acidulated with sulphuric acid, I now used alcohol acidulated with hydrochloric acid, and the pigment was separated on four different occasions; the residue obtained resembled exactly that got by the first method, both as to its appearance and as to its solubility in different media.

The spectra of these solutions differed (as I expected they would) in regard to the band near D, but that at F was exactly the same as before, and acted in the same manner when treated by ammonia and sodic hydrate respectively.

I now proceed to the spectra observed in the solutions of urobilin obtained by this method.

Alcoholic Solution.—An orange-red solution, which in sufficient depth gave the following spectrum:—

Extent, 10 to 32 (or wave-length 678—552)
Band δ, 18 to 20 (or wave-length 620—609)
Band (feeble) ϵ, 22 to 25 (or wave-length 598—582)
} See figure (13).

Shallower depth:—

Extent, 7 to 83 (or wave-length 702—427)
Band, 47 to 54 (or wave-length 502—483)
} See figure (14).

Treated with ammonia the colour of the solution became less red, and the following spectrum was seen:—

Extent, 10 to 40 (or wave-length 678—523)
Band δ, 15 to 19 (or wave-length 640—614)
Band δ, 23 to 26·5 (or wave-length 592—576)
} See figure (15).

In shallower depths the band α of original solution had disappeared.

Ethereal solution.—Of an orange colour. It gave the following spectrum when treated with ammonia:—

$$\left.\begin{array}{l}\text{Extent, 10 to 37 (or wave-length 678—534)}\\ \text{Band }\delta\text{, 16 to 20 (or wave-length 634—609)}\\ \text{Band }\gamma\text{, 23 to 28 (or wave-length 592—568)}\end{array}\right\}\text{See figure (16).}$$

In a less depth the band α at F of original solution had disappeared.

Chloroformic solution.—This solution gave a similar spectrum to, and was altered similarly by, ammonia as the other solutions just described.

Action of Caustic Soda on these solutions.—In appropriate depths caustic soda moves the band at F nearer the red. Thus in the case of the alcoholic solution:—

Band *before* treatment with caustic soda, 47 to 53 (or wave-length 502—485).

Band *after* treatment with caustic soda, 40 to 45·5 (or wave-length 523—506).

And the action of caustic soda on the ethereal and chloroformic solutions was similar. When urobilin is thus treated with caustic soda, ammonia is no longer capable of causing the band to disappear.

Action of Acids on Urobilin prepared as above.—It dissolves in *sulphuric acid*, giving a splendid ruby-red solution, having the following spectrum:—

Extent, 10 to 40 (or wave-length 678—523).

As the bands are uncertain, alcohol was added, when a band appeared from

21 to 25 (or wave-length 604—582),

and a very faint shading from

30 to 35 (or wave-length 560—542) see figure (12). (Band at F in shallow depth.)

It dissolves in *acetic acid*, forming an orange-red solution, giving in suitable depths the following spectrum:—

$$\left.\begin{array}{l}\text{Extent, 10 to 38 (or wave-length 678—530)}\\ \text{Band }\delta\text{, 22 to 26 (or wave-length 598—578)}\\ \text{Band }\gamma\text{, 32 to 35 (or wave-length 552—542)}\end{array}\right\}\text{See figure (17).}$$

Shallow depths:—

Extent, 8 to 85 (or wave-length 694—425).
Band α, 45 to 55 (or wave-length 507—480).

Ammonia develops in this solution three feeble bands:—

Extent, 10 to 41 (or wave-length 678—520).
Band ϵ, 8 to 11 (or wave-length 694—671).
Band η, very feeble, reading could not be taken.
Band δ, 34 to 37 (or wave-length 545—534).

In a slight depth the band at F had disappeared. The pigment was also soluble in nitric acid and in lactic acid; in the former there were no bands, except that at F, visible; in the latter they resembled those of the acetic acid solution.

All the spectra which I have described will suffice to show that the same pigment was evidently present in every solution; there were many more observed and measured, but they were not of sufficient importance to call for their being mentioned here.

It now became necessary to test the urobilin prepared by the hydrochloric acid process for sulphur, which was accordingly done; and not only was sulphur found to be absent, but the presence of chlorine was detected, showing that my inference was correct, and that the sulphur found in the urobilin prepared by the sulphuric acid process was due to the sulphuric acid used in its preparation, and that the chlorine found in the urobilin prepared by the hydrochloric acid process was due to the hydrochloric acid. But in neither case was there free sulphuric acid or free hydrochloric acid, and hence the conclusion follows that the urobilin was in combination with those acids respectively.

I think it will be allowed that the bands visible only at certain depths of the solution belong to the same pigment that gives the band at F, hence they all belong to urobilin. As they are not visible in the aqueous solution, we can understand why they are not visible in urine. And I may also mention that, although water only appears to take up a pigment giving a band at F, yet after evaporation of the water and solution of the residue in alcohol, ether, or chloroform, the same bands again become visible.

Summary.

1. Urobilin has been separated from urine.

2. It has been separated in combination with hydrochloric acid and with sulphuric acid respectively.

3. The spectra of solutions of urobilin obtained by these methods respectively differ in the position of certain feeble bands, but agree in all having a black band at F, which can be made to disappear on adding ammonia in excess, and which is replaced by another band nearer the red end of the spectrum on the addition of sodic hydrate.

4. Urobilin is an amorphous brownish-red pigment, which contains carbon, oxygen, hydrogen, and nitrogen. It is soluble in alcohol, chloroform, acidulated water, acids; partially in ether, benzol, and water, *i.e.*, if the pigment be separated in combination with hydrochloric or sulphuric acid.

5. Urobilin appears capable of existing in different states of oxidation.

6. Urobilin is derived from one of the colouring matters of bile.

7. Urobilin is the colouring matter of the bile of the mouse.

Before concluding this paper I should like to call attention to a peculiarity which the band α of urobilin exhibits: in certain depths it appears very broad, but in less depths it is seen that the portion of the band nearest the violet has disappeared. In other words, the redward part of the band is the most persistent and is the last to disappear on dilution. Thus, taking the alcoholic solution of urobilin prepared by the second process mentioned before, we find that when the extent of the spectrum is 8 to 80 (or wave-length 694—432) this band reads 42—67 (or wave-length 517—453), but when the extent is 7 to 85 (or wave-length 702—425) then the band reads 47 to 53 (or wave-length 502—485.) Urobilin, like hæmaglobin and hæmatin, appears to be a very unstable body, which easily splits up on treatment with reagents into decomposition products, each giving a peculiar spectrum. This accounts for the differences observed in the spectra obtained by the different methods I have described.

EXPLANATION OF FIGURE (p. 27).

1. Solar spectrum.
2. Urobilin, prepared by the alcohol and sulphuric acid method, ethereal solution.
3. The same treated with ammonia.
4. Urobilin, prepared by the same method, dissolved in alcohol.
5. Slight depth of the same.
6. Solution, of which 4 is the spectrum, treated with ammonia.
7. Urobilin, prepared as in 2 and 4, dissolved in chloroform.
8. Shallow depth of the same.
9. Solution, of which 7 is the spectrum, treated with ammonia. Solutions 3, 6, and 9, if examined in a shallow depth, show no band at F.
10. Action of caustic soda on slight depth of solution in 2, 4, and 7.
11. The pigment, prepared by the above method, dissolved in hydrochloric acid.
12. Urobilin, prepared by the hydrochloric acid and alcohol method, dissolved in sulphuric acid; at a shallower depth we get the band at F.
13. Urobilin, prepared by the hydrochloric acid and alcohol method, in alcoholic solution.
14. Shallow depth of same.
15. Solution mapped in 13 treated with ammonia.
16. Ethereal solution of urobilin, prepared as in 13 and treated with ammonia.
17. Urobilin, prepared by the same method, dissolved in acetic acid. At less depth band at F is seen.
18. Spectrum from residue,—got by treatment adopted for separation of urobilin,—from pale straw-coloured urine.

OBITUARY NOTICES OF FELLOWS DECEASED.

DAVID THOMAS ANSTED was born in London on the 5th February, 1814, and after education at a private school, entered the University of Cambridge as a member of Jesus College. He was 32nd Wrangler in the year 1836, and proceeded to the degree of M.A. in due course. About four years afterwards he was elected to a fellowship on the Ley Foundation in his College, which he retained for about eight years. In 1840 he was elected Professor of Geology at King's College, London, which office he resigned in 1853. He was also for some time Lecturer on Geology at Addiscombe, and Professor of Geology at the College of Civil Engineers, Putney. From 1844 to 1847 he was Vice-Secretary of the Geological Society, of which he became a Fellow in 1838. He was elected to the Fellowship of the Royal Society in 1844.

By degrees his attention became diverted from the theoretic to the practical aspect of his favourite study, and for the last thirty years at least of his life he acted professionally as a consulting geologist and mining engineer. For some time before his death he was in failing health, and he expired on the 13th May, 1880, at his residence, Melton, near Woodbridge, Suffolk.

Professor Ansted, however, when systematic teaching ceased to be a part of his regular duties, did not abandon his interest in geology as an educational subject, but not unfrequently lectured and took part in examinations on this and allied sciences. His pen also was rarely for long idle. Besides some contributions to the "Quarterly Journal of the Geological Society," the "Transactions of the Cambridge Philosophical Society," the "Annals and Magazine of Natural History," and other like publications, he was whole or part author of a considerable number of volumes; the majority of these were of a somewhat popular character, and more than one obtained a considerable share of success. He wrote lucidly and pleasantly whether upon matters more directly scientific or upon the incidents of travel. Among these works may be mentioned the following:—"Geological Gossip," first published in 1860; "A Short Trip to Hungary and Transylvania," in 1862; "The Ionian Islands," in the year 1863; and "The Great Stone Book of Nature," published in the same year. He also published a work on geology, in two volumes, in 1844, and more than one smaller book on this subject, physiography, or geography. He was joint author (with Mr. R. G. Latham) of a work on "The

Channel Isles," and wrote, in 1866, upon the "Physical Geography and Geology of Leicester." In the Great Exhibitions of 1851 and 1862 he contributed to the "Reports." His latest and one of his most important works was on "Water and Water Supply," published in 1878. Though for several years, owing to pressure of business and the advance of age, he had ceased to take an active part in the proceedings of the scientific societies of which he was formerly an energetic member, he did not lose that cordiality of manner and kindliness of disposition, which have left a pleasant impression on the memory of his friends.

William Hallowes Miller was born, April 6, 1801, at Velindre, near Llandovery, in Carmarthenshire. At this pretty spot, on the upper part of the strath of the Towey, his father, Captain Miller, had a few years previously fixed his residence. The associations of the family were essentially military. Captain Miller served through a part of the American War; his house was burnt by the rebels, and he was in other respects a heavy loser. All the family papers were destroyed in this conflagration; but Captain Miller is believed to have been a descendant of a distinguished officer who was Adjutant-General to General Wolfe. He was twice married. Of his family by the first wife, two sons were killed in action—one in the act of leading a storming party; another, though crippled by wounds, lived to be a Lieutenant-Colonel, and to receive the distinction of C.B. His daughter also married an officer in the Artillery. Captain Miller, after his return to England, and comparatively late in life, for he was then full sixty years of age, married the daughter of a Welsh clergyman. She was the mother of William Hallowes Miller, and died a few days after his birth.

After receiving his earlier education at private schools, he proceeded to St. John's College, Cambridge, and in the year 1826 graduated, being fifth among the Wranglers in the Mathematical Tripos. In the earlier part of 1829 he was elected a Fellow of his College, and proceeded in due course to the degree of M.A. For some time he filled the office of a College tutor, and his first literary work was mathematical—a "Treatise on Hydrostatics," published in 1831, and followed a few years subsequently by one on "Hydrodynamics." The two were afterwards republished in one volume, and formed for many years the chief text-book on that subject in the Cambridge lecture-rooms. This treatise is conspicuous for its exactness and lucidity, though its terseness—a distinguishing feature in all the author's writings—makes it a rather difficult book for a student of only average ability. At this time, the Chair of Mineralogy at Cambridge was occupied by Dr. Whewell, who, on his election to that office in the year 1828, had thrown himself energetically into its duties, and had especially de-

voted himself to the study of crystallography. Miller was attracted to the same subject; and four years later, when Professor Whewell resigned, he used his influence to obtain the chair for his pupil. Thus, in the year 1832, Miller was elected to the post, the duties of which became the chief work of his long and laborious life.

In 1838 Professor Miller was elected a Fellow of the Royal Society. In 1841 he proceeded to the degree of Doctor of Medicine. To this temporary diversion from more congenial studies he was compelled by the statutes which at that time governed the College. These required that all the Fellows, after a certain time, should be in Holy Orders, with the exception of four, two of whom were to be students of medicine. To one of these Fellowships Professor Miller was transferred in the year 1834; but it is needless to add that, though he complied with the requirements of the statute, he made no attempt to follow medicine as a profession. In 1844 he vacated his Fellowship at the College, by marriage, in accordance with the existing statutes. However, thirty years afterwards he was again elected a Fellow of his old College, under the statute (granted in 1860) empowering the Society to elect as Fellows persons eminent for science or learning, though in other respects technically disqualified. But Professor Miller's work was now drawing near its end. From his youth he had been a hard worker, and had lived perhaps almost too sparingly. He delivered his lectures as usual in the earlier part of 1876, but a change in the expression of his face began to be rather marked, which seemed to forebode a giving way of his robust constitution, and caused anxiety to his friends. Their fears were not groundless. In the October term of 1876 a short course of lectures which he had announced was interrupted by a slight stroke of paralysis. This proved the beginning of the end. He was never able to meet his class again, and the duties of the chair were henceforth discharged by a deputy. Very slowly, but very surely, his vital powers declined—a torpor stealing alike over mind and body—till at last he fell asleep on the 20th May of the present year (1880).

Professor Miller's name is inseparably connected with two important branches of scientific work. The first of these belongs, as might be expected, to mineralogy. "Crystallography," as it has been said, "was Miller's science. It had taken its first shape in the hands of Haüy in the decade of years before he was born, and in those of Weiss, of Mohs, and especially of Franz Ernst Neumann and of Grassmann; it had been receiving development during the years of Miller's youth and manhood." To this his predecessor, Professor Whewell, had contributed by an important memoir on the geometrical treatment of crystal forms, published in the "Transactions of the Cambridge Philosophical Society." "Taking this memoir and Neumann's treatise of 1823 ('Beiträge zur Krystallonomie') as his starting point,

Miller proceeded to develop a system of crystallography, which was not published till 1838, but which was the most important work of his life." His system represented the face of a crystal by a symbol composed of three numerals, or indices. Selecting three crystallographic axes, parallel respectively to possible edges of a crystal, and a face of that crystal making certain intercepts on these axes, and taking the three simplest whole numbers (a, b, c, suppose) whose ratio expressed the ratio of these intercepts, he expressed the ratio of the intercepts of any other face of the crystal, by multiplying a, b, c respectively by $\frac{1}{h}, \frac{1}{k}, \frac{1}{l}$ respectively, where h, k, l were integers, and formed the symbol of the new plane.

"The elegant way" (to continue the words of Professor Maskelynė, already quoted) "in which this mode of representing a face lent itself to yielding expressions for the relations between faces belonging to a zone (*i.e.*, faces that would intersect in edges parallel to the same line) gave it a superiority over previous methods, due to its bringing the symbols of the crystallographer into a form similar to that employed in algebraic geometry. Miller's work consisted in working out into a beautiful system the indicial method of notation and calculation in crystallography, and obtaining expressions adapted for logarithmic calculations by processes of great elegance and simplicity. Miller's system, then, gave expressions for working all the problems that a crystal can present, and it gave them in a form that appealed at once to the sense of symmetry and appropriateness of the mathematician." He thus, as it has been well said, "placed the keystone into the arch of the science of crystallography," and the "future development of that science, there can be little doubt, will follow on the lines laid down by Miller."

Professor Miller's shorter communications on mineralogy and physics are numerous and valuable, and, in addition to them and to his original treatise, he published, in 1863, a tract on crystallography. In 1852 a work appeared entitled a new edition of the "Elementary Introduction to Mineralogy, by the late William Phillips," by H. J. Brooke and W. H. Miller. It is, however, no disparagement to either the original author or his fellow editor to say that Professor Miller made this volume almost his own. As has been said by the authority quoted above, "The publication of this severe little volume was an epoch in the science which it illustrated; it contained a mass of results obtained by Miller with all his accuracy and all his patience through many years, and tabulated in his usual concise manner. It is a monument to Miller's name, though he almost expunged that name from it."

But Professor Miller's reputation does not rest only upon his work as a mineralogist, great though that was. His name is no less in-

separably connected with the difficult and delicate experiments and investigations connected with the restoration of the standards of measurement and weight, and with the subsequent labours of the International Metric Commission.

After the fire which, in 1834, consumed the Houses of Parliament, it was found that the standards of measurement and weight there preserved were hopelessly ruined, and a Commission was appointed to consider the questions connected with their restoration. Professor Miller was not, indeed, a member of that Commission, but it is well known that his friendly assistance contributed greatly to guide the Commission in some of their more important recommendations, especially in those which related to the means to be provided for contingent restoration of the standard of weight. In sections 3 and 5 of the Act 5th George IV, it was directed that "in case of the loss of the standard, the yard shall be restored by taking the length which shall bear a certain proportion to the length of the pendulum, vibrating seconds of mean time in the latitude of London, in a vacuum, at the level of the sea; and that the pound shall be restored by taking the weight which bears a certain proportion to the weight of a cubic inch of water weighed in a certain manner." In their report, dated December 21, 1841, the Commissioners decline to recommend the adoption of these provisions, for reasons which are given therein, and advise that each standard should be restored from measures and from weights then existing, which had been most carefully compared with the original standards, stating also that they were "fully persuaded that, with reasonable precautions, it may always be possible to provide for the accurate restoration of standards by means of material copies, which have been carefully compared with them, more securely than by reference to experiments referring to natural constants."

In 1843 a Committee was appointed to superintend the construction of the new Parliamentary standards of length and weight, of which Professor Miller was a member, and to him was confided the construction of the new standards of weight. In the "Philosophical Transactions" for 1856 he describes at length "The operations for restoring the value of the Old Standard of Weight, for constructing the New Standard of a different value, for constructing various derived Standards, and for establishing the relative value of the Kilogramme," a paper which (to quote the words of the Astronomer Royal, endorsed by a former President of this Society, Sir Edward Sabine) "will long be cited as a model of accuracy."

He was subsequently a member of a new Royal Commission for "examining into and reporting on the state of the secondary standards, and for considering every question which could affect the primary, secondary, and local standards."

In the year 1870 he was appointed a member of the Commission Internationale du Mètre. This led to various visits to Paris between the above year and 1874. More than one of his colleagues has expressed in the warmest terms his sense of the value of Professor Miller's services to that Commission. Perhaps there was no member whose opinions had greater weight in influencing a decision upon any intricate and difficult question.

His services to this Society must not be forgotten in a sketch of his life, however brief. Elected a Fellow in the year 1838, he was appointed Foreign Secretary in 1856, a post for which he was eminently fitted by his accurate and extensive knowledge of French, German, and Italian, his methodical habits, and unvarying courtesy, as well as by his extensive scientific knowledge.

To those who enjoyed Professor Miller's friendship three characteristics were conspicuous above all. One was the extent and the accuracy of his knowledge. Not only on those subjects to which he had more notably devoted himself, but on almost any question of physical science he was an authority. Younger men in Cambridge looked upon him as a kind of living encyclopædia, for their questions were at once met by answers, clear, accurate, and concise. As one of the most competent judges now living in that University writes, "There was, I think, no person in Cambridge whose knowledge of natural philosophy on the whole exceeded, or even equalled, that of Professor Miller. He kept up his reading to an extent that was surprising."

Another was the simplicity of his disposition. Seldom has there been a man with so few wants. To all except himself he was generous; hospitable to his friends, he was almost Spartan in his own habits. This trait combined with the remarkable inventiveness of his mind, is evidenced even in his laboratory. There the visitor is surprised to find the most homely odds and ends utilised in the construction of instruments capable of performing delicate measurements. "Give Professor Miller," a friend has said, "some loose lenses, bits of glass tubing, laths, copper wire, and especially some pill-boxes, and he will make any instrument that he wants."

The third characteristic was the remarkable combination which he exhibited of independence of thought and freedom of opinion, with gentleness of temper and speech, with forbearance, courtesy, and respect for the opinion of others. No one, however conscious of inferiority, whether in experience or in mental powers, had any reason to fear to speak in the presence of Professor Miller. His words were sure to be received with consideration, often almost with deference, beyond their deserts.

His life's course was tranquil and full of quiet happiness. Until his last illness he enjoyed excellent health. Year after year was spent in the labours—to him a pleasure—which have been briefly noticed, with

an occasional visit to the Continent, either for duty or for relaxation. He delighted especially in the scenery of the dolomite mountains of the Italian Tyrol, spending among them many hours of quiet enjoyment, while their magnificent outlines were recorded with rare fidelity by the accomplished companion of his life.

Happy, then, in his domestic life, happy in the affectionate appreciation of numerous friends of varied ages and ranks, he was also happy in seeing his work (though for honours and rewards he cared less than most men) not unacknowledged by his contemporaries. In addition to the honours mentioned above, he received in 1865 the degree of LL.D. from the University of Dublin, and in 1876 that of D.C.L. from Oxford. In 1870 he was awarded a Royal Medal by this Society. He was a Knight of the Order of St. Maurice and St. Lazare of Italy and of the Order of Leopold of Belgium. He was also an honorary member of the Royal Society of Edinburgh, of the Mineralogical Society of France, and of Boston, U.S.A., a foreign member of the Mineralogical Society of St. Petersburg, of the Imperial Royal Academy of Sciences, Vienna, and of the Royal Society, Göttingen; and a corresponding member of the Academies of Berlin, Munich, Paris, St. Petersburg, and of Turin.

GOVERNMENT GRANT OF £1000.

A Meeting of the Government-Grant Committee will be held in February, 1881. It is requested that applications to be considered at that Meeting be forwarded to the Secretaries of the Royal Society, Burlington House, before the 31st December, 1880.

GOVERNMENT FUND OF £4000

FOR THE PROMOTION OF SCIENTIFIC RESEARCH.

A Meeting of the Government-Fund Committee will be held in February, 1881. It is requested that applications to be considered at that Meeting be forwarded to the Secretaries of the Royal Society, Burlington House, before the 31st of December, 1880.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,
AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

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November 18, 1880.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

General Boileau, Mr. Currey, Mr. De La Rue, Mr. Hudson, and Mr. Matthey, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Essential Properties and Chemical Character of Beryllium (Glucinum)." By L. F. NILSON and OTTO PETTERSSON. Communicated by WARREN DE LA RUE, D.C.L., F.R.S. Received June 21, 1880.

In this paper we wish to call attention to some experimental facts which may give a clue to the real nature of beryllium, an element which since the beginning of this century has been the enigma of inorganic chemistry.

The oxide of beryllium was discovered in 1795 by Vauquelin. It was considered a monoxide, BeO, until 1815, when Berzelius* ranged it, principally on account of its basic sulphates, among the sesquioxides. The weighty reasons for this arrangement, never since refuted, which Berzelius added later in the fifth edition of his "Lehrbuch d. Chemie," p. 1225, are too well known to be recorded here.

In 1842 Awdéeff† analysed the double sulphates and fluorides of beryllium, which showed qualities not agreeing entirely with the analogous compounds of aluminium, iron, chromium, &c. As the formula of these compounds could be written

$$\bar{\mathrm{K}}\mathrm{O,SO_3+BeO,SO_3,}$$
$$\bar{\mathrm{K}}\bar{\mathrm{F}}\mathrm{l+Be}\bar{\mathrm{F}}\mathrm{l,}$$

this was considered by the chemists of that time to support strongly the old theory. Still the classification of Berzelius prevailed and was confirmed by H. Rose,‡ who showed the correspondence of the mole-

* "Schweigg. Journ. f. Ch. u. Ph.," xv, p. 296.
† "Pogg. Ann.," lvi, p. 101.
‡ "Pogg. Ann.," lxxiv, p. 429.

cular volumes of Be_2O_3 with Al_2O_3, and also by Ebelmen,* who obtained the oxide of beryllium in crystals isomorphous with Al_2O_3. Subsequently an elaborate work, "De Glucium et de ses Composées," 1855, by Debray,† once more caused a change in the prevalent opinion.

Debray regarded beryllia as an isolated member of the series occupying a position intermediate between the monoxides and the sesquioxides, and showing marked analogies with both groups, but not intimately connected with either of them by isomorphism. As the analyses of its compounds in most cases agreed better with the simple formula BeO, this was preferable to Be_2O_3.

Klatzo,‡ in 1868, endeavoured to decide this matter finally by the assumption of a complete isomorphism between the sulphates of Be and Mg, Co, Fe, Ni, and though Marignac,§ in 1873, proved that this pretended isomorphism did not exist, and was founded on a grave mistake, the opinion that beryllia was a monoxide was at this time universally accepted by chemists. Theoretically this opinion was founded on the "periodic law" of Mendeléeff.|| The classification of beryllium at the head of the second group among the diatomic elements is a leading point in the theory of Mendeléeff.

If the composition of beryllia was Be_2O_3, and the atomic weight of beryllium=13·8, instead of 9·2, the place of Be=9·2 in the system would be vacant, and the order of the series partially reversed, the "atomic analogies" would be overthrown, and, still worse, beryllium =13·8 would find no place at all in the system, except in the fifth group among the five-atomic elements, to which it certainly does not belong.

The final decisive proof still wanting to confirm the ideas of Mendeléeff's was furnished, 1877, by Reynolds,¶ who found the specific heat of metallic beryllium=0·642, which (Be=9·2) makes the atomic heat=5·9, in accordance with the law of Dulong and Petit.

About a year before the publication of Mr. Reynolds, we had succeeded, by means of a new method, in isolating metallic beryllium from its chloride. We employed** a massive crucible of wrought iron, hermetically closed by a screw-plug, wherein equivalent quantities of beryllium chloride and metallic sodium were heated to redness. Metallic beryllium was thus obtained, partly fused into globules, partly forming aggregations of little prismatic crystals, which in brightness

* "Ann. d. Ch. u. Pharm.," lxxx, p. 211.

† "Ann. de Chimie et de Phys.," [3], xliv, p. 5.

‡ "Ueber die Constitution der Beryllerde." Dorpat, 1868. "Journ. f. Prakt. Ch.," cvi, p. 227.

§ "Ann. de Chim. et de Phys.," [4], xxx, p. 45.

|| "Ann. d. Ch. u. Pharm., Suppl.," viii, p. 151 (1871).

¶ "Phil. Mag.," [5], iii, p. 38.

** For the details of the experiment see "Darstellung und Valenz des Berylliums," "Pogg. Ann.," [2], iv, p. 554 (1878).

and colour resembled needles of polished steel. The metal was, as might be expected, not absolutely pure. The analysis was somewhat difficult, the mean result of a number of accordant determinations being:—

Be (metallic)............	86·94	per cent.
Be_2O_3....................	9·99	„
Fe.....................	2·08	„
SiO_2....................	0·99	„
	100·00	

We next determined its specific heat by the method of Bunsen (ice-calorimeter). We here met with quite unexpected difficulties; but, having given up the original arrangement of the experiment described by Bunsen* as impracticable, we, by means of an arrangement similar to that recommended by Schuller and Wartha,† obtained the following results:—‡

Specific heat of Be=0·4084 between 0° and 100°.
Atomic heat of Be=5·64 [Be=13·8].

These results, not in accordance either with those recently obtained by Mr. Reynolds or the periodic law of Mendeléeff, were not accepted without hesitation by chemists. Notwithstanding that, the editor of the new edition of Gmelin's "Handbuch d. Chemie," Professor Kraut, altered the formulæ of beryllia and its compounds in the part of the great encyclopædia of chemistry then passing through the press, several other chemists publicly or privately commented on our work, and urged us to pursue our inquiries further. Among the objections thus made we deem the following most worthy of discussion.

Mr. Lothar Meyer § hints that the equivalent of beryllium may be wrong, and suggests a revision of that number. If it should be found lower than 4·0, instead of higher, beryllium may still be considered a three-atomic element, without upsetting the periodic law. In that case it would only be necessary to interpose a new group of metals between the trivalent and quadrivalent elements. We had no reason to doubt the accuracy of the old number (=4·6), which we had found and verified by many analyses, but we redeemed our promise to Mr. Lothar Meyer,|| and undertook the following determinations of the atomic weight of beryllium.

In order to determine this value with the utmost accuracy, we thought it safest to choose the simplest possible method, viz., the

* "Pogg. Ann.," cxli, p. 1.

† "Pogg. Ann.," [2], ii, p. 359.

‡ *Obs.*—Allowance is made for the impurities of the metal. The specific heat of Be_2O_3 was found =0·2471 between 0—100° C.

§ "Ber. d. Deutsch. Chem. Gesellsch.," xi, p. 576.

|| *Ibid.*, p. 906.

analysis of its sulphate. Originally we also thought of analysing the chloride by titration with silver nitrate, but having found that sublimed beryllium chloride could not be obtained entirely pure, on account of its corrosive action on glass, we gave up the idea.

Sulphate of beryllium is undoubtedly a neutral salt, and is easily obtained in beautiful crystals, which do not change in the air; but at 100—110° C. it loses half of its water, at 250° it becomes anhydrous, and after heating to light redness pure beryllia remains. Still there is a difficulty in the analysis; for anhydrous sulphate and pure beryllia, as obtained by calcination, are both very hygroscopic substances. We therefore chose the hydrated sulphate, which could be weighed with the greatest accuracy, as the most fitting substance to start with; this salt allowing pulverisation and pressure* without losing a trace of its constituent water. The sulphate was prepared by heating to dryness an aqueous solution of sublimed chloride with an excess of pure sulphuric acid. By repeated crystallisation the sulphate could easily be purified from a slight trace of calcium sulphate, originating from the action of the gaseous chloride on the glass tubes.

The analyses I, II, refer to beryllium sulphate obtained in this manner; for III, IV, the chloride was precipitated with ammonia and the hydrate treated with sulphuric acid. The sulphate was repeatedly crystallised. The difficulty in the analysis is the weighing of the calcined beryllia. For this purpose the crucible, still red hot, was placed in an exsiccator filled with anhydrous phosphoric acid, and after cooling placed immediately on the scale pan of a Bunge's balance, the equilibrium being beforehand approximately established. In this manner the whole operation required only a few seconds.

By spectroscopical test Professor Thalén has found the beryllia used for these determinations to be absolutely pure.

The hydrated beryllium sulphate has given in the determinations, thus executed, the following values:—

Experiment.	Weighed Sulphate.	Loss of water at 100° C.		Loss of water and SO_3.		Beryllia.		Equivalent of beryllium.	
	grs.	gr.	per cent.	grs.	per cent.	gr.	per cent.	O = 8	O = 7·98
I	3·8014	0·7696	20·245	3·2627	85·829	0·5387	14·171	4·556	4·544
II	2·6092	0·5282	20·244	2·2395	85·831	0·3697	14·169	4·552	4·542
III	4·3072	—	—	3·6973	85·840	0·6099	14·160	4·545	4·533
IV	3·0091	—	—	2·5825	85·824	0·4266	14·176	4·557	4·550
Mean	—	—	—	—	85·831	—	14·169	4·552	4·542

* It was pressed between sheets of fine, porous, bibulous paper, the surface of which had been previously smoothed by heavy pressure.

The equivalent of beryllium has hitherto been determined by—

Berzelius	=	4·9	(analysis of the sulphate).
Awdéeff	=	4·71	,, ,, ,,
		4·93	(analysis of the chloride).
Weeren	=	4·62	(analysis of the sulphate).
Klatzo	=	4·59	,, ,, ,,
Debray	=	4·61—4·71	(analysis of the oxalate).
Nilson and Pettersson	=	4·552.....	(analysis of the sulphate).

All these numbers are higher than 4·0, and consequently the atomic weight of beryllium, if trivalent, must be 13·65, consequently higher than that of carbon. The before-mentioned supposition is consequently proved to be unfounded.

Mr. Lothar Meyer further observes that the atomic heat of the oxygen in beryllia, if a sesquioxide, would be less than in any other oxide known. In the next paper we give the whole series of our determinations of the molecular heats of the rare earths and their sulphates. From this survey, which shows that beryllia, with regard to heat and volume, occupies its proper place at the head of the sesquioxides, we here only extract a few determinations.* If c signifies the specific heat,—

			Atom. heat of oxygen.
In	Be_2O_3	$c = 75{\cdot}32 \,.\, 0{\cdot}2471 = 18{\cdot}61$	
	Be_2	$c = 27{\cdot}32 \,.\, 0{\cdot}4246 = 11{\cdot}60$	
		$7{\cdot}01 = 3.$	$2{\cdot}34$
,,	Al_2O_3	$c = 102{\cdot}8 \,.\, 0{\cdot}1825 = 18{\cdot}78$	
	Al_2	$c = 54{\cdot}8 \,.\, 0{\cdot}2143 = 11{\cdot}74$	
		$7{\cdot}04 = 3.$	$2{\cdot}35$
,,	Sc_2O_3	$c = 136{\cdot}0 \,.\, 0{\cdot}1530 = 20{\cdot}81$	
	Sc_2	$c = 88{\cdot}0 \,.\, 0{\cdot}1454 = 12{\cdot}80$†	
		$8{\cdot}01 = 3.$	$2{\cdot}67$

* Our determinations refer to pure oxides obtained by chemical operations. They are also strictly corresponding and comparable. Other determinations with native alumina (sapphire), made by different methods (Regnault, Neumann), gave a higher number. Determinations by means of the ice-calorimeter always give smaller results, because the standard measure for the heat developed is greater (1 calory = mean of the specific heat of water between 0—100° C.).

† The atomic heat of scandium is supposed to be = 6·4, according to the law of Dulong and Petit.

			Atom. heat of oxygen.
In Ga_2O_3	$c = 184 \,.\, 0{\cdot}1062 = 19{\cdot}54$		
Ga_2	$c = 136 \,.\, 0{\cdot}0802 = 10{\cdot}91$		
	$8{\cdot}63 = 3.$		$2{\cdot}88$
„ In_2O_3	$c = 274{\cdot}8 \,.\, 0{\cdot}0807 = 22{\cdot}17$		
In_2	$c = 226{\cdot}8 \,.\, 0{\cdot}0570 = 12{\cdot}92$		
	$9{\cdot}25 = 3.$		$3{\cdot}08$

According to the determinations made by us under identical conditions, and therefore strictly comparable, the atomic heat of oxygen in beryllia$=Be_2O_3$ is the same as that in alumina; this, however, can by no means be considered as exceptional. Alumina and beryllia are the leading members of a group of sesquioxides, wherein the atomic heat and (as will be seen from the following paper) the atomic volume of oxygen increases with increasing values of the atomic weights of the metals.

We will now refer to another objection raised to our former researches. Mr. Brauner* admits the specific heat of beryllium to be 0·4084 between 0—100°, but supposes that it may rapidly increase with the temperature in the same way as does the specific heat of C and Bo. If this were the case, he thinks beryllia could be BeO, the atomic heat of beryllium$=3{\cdot}76$ between 0—100°, and normal$=6{\cdot}4$ first at a much higher temperature. In our detailed paper, we have tried to meet such an objection, by pointing out that no *metallic* element is as yet known, the atomic heat of which does not agree with the law of Dulong and Petit. However, in order to remove any doubt in this respect, we have determined the specific heat of beryllium at different temperatures lying between 0—300° C. We sifted the metal used in our former determinations through a gauze of platinum, the holes of which were 0·25 sq. millim. For the following experiments we used only that part which did not pass through the gauze, on the supposition that this, consisting of globules and larger crystals, was the purer metal. The analysis confirmed this opinion, for the composition was found to be—

Beryllium	94·41
Beryllia	4·89
Iron	0·70
	100·00

The following table gives only the results, not the details of our

* "Ber. d. Deutsch. Chem. Gesellsch.," xi, 1872.

determinations;* the values are referred to pure beryllium, making allowance for the impurities of beryllia and iron:—

Temperature.	Specific heat.	Atomic heat.	
0°— 46°·30 C.	0·3959	5·46	Heating in the vapour of CS_2.
0— 46·30	0·3950		
0— 46·30	0·3980		
0— 46·50	0·4005		
0—100·18	0·4250	5·79	Heating in the vapour of water.
0— 99·97	0·4242		
0—214·0	0·4749	6·48	Heating in the vapour of nitrobenzol.
0—214·0	0·4751		
0—299·5	0·5054	6·90	Heating in the vapour of diphenylamine.
0—299·5	0·5066		

Thus the specific and atomic heat of beryllium increase with the temperature, but a comparison with the same numbers for iron between 0—300° C. shows that such an increase of these values is not unusual.

	0—100°.		0—300°.		Authority.
	Specific heat.	Atomic heat.	Specific heat.	Atomic heat.	
Iron........	0·1124	6·29	0·1266	7·09	Bède.
Beryllium...	0·4246	5·79	0·5060	6·90	Nilson and Pettersson.

Between 0—100° C., the atomic heat of beryllium is equal to that of aluminium=5·87, and gallium=5·59, at 214° C. it is normal=6·48, and at 300° C. it has attained the same value as iron at the same temperature. Beryllium can thus certainly not be compared with the diamond in this respect, the specific heat of which, according to the researches of Weber, being many times higher at 0—300° C. than at 0—100° C.†

Every doubt as to the real atomic weight of beryllium must be

* We have also been obliged to determine the increase of the specific heat of beryllia and platinum by higher temperatures in the same way as that of beryllium itself, the metal for the experiments being enclosed in little capsules of platinum foil, hermetically soldered with chemically pure gold. Glass tubes cannot be employed at higher temperatures than 100° for two reasons: 1st, the glass would crack when suddenly cooled to 0°; and 2nd, its specific heat increases very rapidly.

† "Ber. d. Deutsch. Chem. Gesellsch.," v, 303.

removed by the results above mentioned. Fixed at 13·65, according to our determinations, the value of its atomic heat becomes perfectly harmonious with the law of Dulong and Petit.

We will, in conclusion, say a few words upon a paper wherein the results of our former researches have been criticised. In the "Proceedings of the Royal Society," 1879, Mr. Carnelley applies a new method of calculating the fusion points of halogen compounds, and applying it to those of beryllium, argues in the following manner:—

Beryllium must be either a dyad or a triad, and must belong either to the second or to the third group of Mendeléeff's series; if a dyad or Be=9·2, its chloride, $BeCl_2$, can be calculated to fuse at +547— +600° C, which is confirmed by experiment; if a triad or Be=13·8, the chloride, $BeCl_3$ or Be_2Cl_6, ought to fuse about 500 degrees lower, *i.e.*, at +50°—+100° C., which it obviously does not; *ergo*, beryllium is a dyad, and Be=9·2.

We presume that Mr. Carnelley's knowledge of the physical properties of the triads is, like our own, very limited. With the exception of aluminium, we really know little or nothing of the melting or boiling points of chlorides, bromides, and iodides belonging to this group, and we think analogies taken only from one member, aluminium, to be too narrow a base for a calculation which is meant to apply to the whole group. There may be chlorides, bromides, and iodides which do not behave like Al_2Cl_6, in regard to boiling and fusion points. We will, in the following paper, give reasons for our opinion that beryllium and aluminium are each leading members of two different groups of trivalent metals. The nearest relatives of beryllium among these are neither calcium and magnesium, with which it has, in fact, little or nothing in common, nor aluminium, with which it has very much more in common, but rather the rare elements, scandium, yttrium, erbium, and ytterbium. We believe that Mr. Carnelley ought to try his calculation on the halogen compounds of the rare elements before asserting "that Nilson's and Pettersson's determination of the specific heat of beryllium must be incorrect." If the properties of the halogen compounds of these elements should be found to agree with the calculation, then we confess that the matter becomes somewhat uncertain, for then one will have to choose between the law of Dulong and Petit and that of Carnelley.*

Our above-mentioned experimental researches, confirmed still more by the experiments, which will be quoted in a second paper, lead us to the conclusion that the real atomic weight of beryllium is=13·65. But with this value the periodic law cannot admit this element among the metals nearest related, and this fact obviously militates against its

* "Ber. d. Deutsch. Chem. Gesellsch.," v, 303.

general applicability. Before concluding this memoir, we will just point out that this is not an isolated case of its kind. For the element which should take its place between Sb=122 and I=127, the periodic law requires an atomic weight=125; with regard to its general properties tellurium is quite admissible in this place, but its atomic weight=128 is too high. Although this number was the result of the determinations of Berzelius and v. Hauer, this want of accordance with the periodic law induced Willis* to make a new determination, but he only confirmed the former results. Thus neither tellurium nor beryllium can be fitted into Mendeléeff's system. And further, after Councler's† discovery of the boroxychloride, $BoOCl_3$, boron may be considered as five-atomic, but it certainly cannot be placed among elements of that valence; and when once the chemistry of the rare earth-metals shall be made clear, where can be placed all these elements, the number of which has already become very great and doubtless will be still augmented? Already erbium and ytterbium, with the now fixed atomic weights of 166‡ and 173,§ for the pure metals (the earths=Er_2O_3 and Yb_2O_3), can scarcely be ranged in Mendeléeff's system in places indicated by their relation to the other earth-metals or by their "atomic analogies."

In consequence of what has been indicated here, the periodic law in its present condition cannot be said to be quite an adequate expression for our knowledge of the elements; this theory, however, having given the most striking proofs that the truth in many respects has been found (as for example: the new formulæ for the rare earths $=R_2O_3$ instead of RO, and the discovery of gallium and scandium, the existence of which the law has foreseen in the elements eka-aluminium and eka-boron), we may expect that the periodic law may be so modified and developed that it can embrace and explain every fact, stated by experiment.

* "Liebig's Ann. d. Ch.," ccii, p. 242.

† "Ber. d. Deutsch. Chem. Gesellsch.," xi, p. 1108.

‡ According to Cleve.

§ According to Nilson.

II. "On the Molecular Heat and Volume of the Rare Earths and their Sulphates." By L. F. NILSON and OTTO PETTERSSON. Communicated by WARREN DE LA RUE, D.C.L., F.R.S. Received June 21, 1880.

At the request of the Royal Academy of Sciences at Stockholm, we some years ago undertook an extended research into the physical properties of the rare earth-metals and their compounds. Having performed the laborious task of separating and purifying their oxides, we are now able to publish our first series of determinations concerning their principal properties, which chemically are of the greatest importance, viz., molecular heat and molecular volume.

The rare earths, with a few exceptions (CeO_2, ThO_2, ZrO_2), belong to a group of sesquioxides. In order to obtain a larger number for comparison, we have extended our research not only to the rare earths, but also to some other nearly related compounds, the molecular heat and volume of which were hitherto unknown. As to these values, previously known for some other oxides, Al_2O_3, Fe_2O_3, Cr_2O_3, according to Regnault, Kopp, and others, we have already observed in our preceding paper "On the Essential Properties and Chemical Character of Beryllium," that only such determinations are strictly comparable which are made by the same method, under the same circumstances, and referred to the same unit of measure.

The specific densities and molecular volumes, which are given in the following tables, are obtained by means of a method, specially adapted to prevent the errors arising from adhesion of air to pulverulent substances.* The densities taken by this method will therefore generally be found a little higher and the molecular volumes a little lower than the numbers usually given by others.

The specific heats of the different compounds are determined with Bunsen's ice-calorimeter, by means of the same process which we have more fully described in our detailed memoir on beryllium.† All the numbers given are the means of at least two determinations, which agree perfectly, the experiments being made under the most favourable circumstances. As to the values obtained, we beg to observe that the specific and molecular heat will be found a little smaller by the ice-melting method than by other methods, on account of the different unit of comparison.

The molecular weight of the different oxides was determined by a special analysis or synthesis of the sulphates, and the very same chemi-

* For the details, see Otto Pettersson, "Molecularvolumina einiger Reihen von isomorphen Salzen," in "Nov. Act. reg. Soc. Scient. Ups.," ser. iii. Upsala, 1873.

† "Pogg. Ann.," [2], iv, p. 554.

cally pure substances were employed both for the thermic, volumetric, and magnetic experiments. The results only and not the details of these experiments are given in the tables.

Oxides.

Compounds.	Formula.	Molecular weight.	Specific weight.	Specific heat.	Molecular heat.	Molecular volume.
Beryllium oxide ..	Be_2O_3....	75·3	3·016	0·2471	18·61	24·97
Aluminium oxide..	Al_2O_3....	102·8	3·990	0·1827	18·78	25·76
Sapphire cryst.....	Al_2O_3....	102·8	3·990	0·1879	19·32	25·76
Chrysoberyl cryst..	$\left.\begin{matrix}Al_{\frac{4}{3}}\\ Be_{\frac{1}{2}}\end{matrix}\right\}O_3$..	95·9	3·734	0·2004	19·22	25·69
Scandium oxide* ..	Sc_2O_3....	136·0	3·864	0·1530	20·81	35·19
Gallium oxide† ...	Ga_2O_3 ...	184·0	—	0·1060	19·50	—
Yttrium oxide	Y_2O_3	227·0	5·046	0·1026	23·29	44·99
Indium oxide.....	In_2O_3....	274·8	7·179	0·0807	22·17	38·28
Erbium oxide‡....	Er_2O_3 ...	380·0	8·640	0·0650	24·70	43·98
Ytterbium oxide ..	Yb_2O_3 ...	394·0	9·175	0·0646	25·45	42·94
Lanthanum oxide .	La_2O_3....	326·0	6·480	0·0749	24·42	50·31
Didymium oxide ..	Di_2O_3....	341·0	6·950	0·0810	27·62	49·07
Zirconium oxide...	ZrO_2	122·0	5·850	0·1076	13·13	20·86
Cerium bioxide ...	CeO_2	171·5	6·739	0·0877	15·04	25·45
Thorium oxide....	ThO_2....	264·0	9·861	0·0548	14·47	26·77

Hitherto it was supposed to be a general rule, that the atomic heat of oxygen in any oxide was not less than 3·5 or greater than 5·1. The numbers above given compel us, however, to assign a still lower value to the atomic heat of oxygen in alumina and beryllia, viz., 2·34.§ Into these oxides it enters with a minimum capacity of heat and volume. From these earths upwards the molecular heat and volume of the sesquioxides gradually increase with increasing molecular weights.

The following table shows that the molecular heat (1) of Er_2O_3, Yb_2O_3, La_2O_3, and Di_2O_3 nearly agrees with those for other sesquioxides, determined by Regnault; (2) that of Be_2O_3, approaches to the same for alumina; and (3) that of ZrO_2, CeO_2, and ThO_2 is as high as the values for SnO_2, TiO_2, and zircon, determined by the same author, and for MnO_2 according to Kopp. The validity of Neumann's law receives hereby a new confirmation.

* According to Nilson.

† For the oxide employed we are under obligation to M. Lecoq de Boisbaudran, who placed 0·138 grm. gallium at our disposal.

‡ Professor Cleve has kindly placed at our disposal the purest erbia he has been able to obtain.

§ See the preceding paper, page 41.

Formula.	Molecular weight.	Specific heat.	Molecular heat.	Authority.
Bo_2O_3	69·8	0·2374	16·57	Regnault
Cr_2O_3	152·4	0·1796	27·37	„
Fe_2O_3	160·0	0·1681	26·90	„
As_2O_3	198·0	0·1279	25·32	„
Sb_2O_3	292·0	0·0901	26·31	„
Bi_2O_3	468·0	0·0605	28·31	„
TiO_2	82·0	0·1703	13·97	„
$\left.\begin{matrix}Si_{\frac{1}{2}}\\Zr_{\frac{1}{2}}\end{matrix}\right\}O_2$	90·8	0·1456	13·22	„
SnO_2	150·0	0·0933	14·00	„
MnO_2	87·0	0·1590	13·83	H. Kopp

Anhydrous Sulphates.

Compounds.	Formula.	Molecular weight.	Specific weight.	Specific heat.	Molecular heat.	Molecular volume.
Beryllium sulphate...	Be_23SO_4	315·3	2·443	0·1978	62·37	129·07
Aluminium sulphate..	Al_23SO_4	342·8	2·710	0·1855	63·59	126·50
Scandium sulphate...	Sc_23SO_4	376·0	2·579	0·1639	62·42	145·80
Chromium sulphate ..	Cr_23SO_4	392·4	3·012	0·1718	67·41	130·27
Ferric sulphate	Fe_23SO_4	400·0	3·097	0·1656	66·24	129·16
Gallium sulphate.....	Ga_23SO_4	424·0	—	0·1460	61·90	—
Yttrium sulphate	Y_23SO_4	467·0	2·612	0·1319	61·60	178·80
Indium sulphate.....	In_23SO_4	514·8	3·438	0·1290	66·41	149·77
Lanthanum sulphate..	La_23SO_4	566·0	3·600	0·1182	66·90	157·22
Cerium sulphate	Ce_23SO_4	567·0	3·912	0·1168	66·23	144·94
Didymium sulphate ..	Di_23SO_4	581·0	3·735	0·1187	68·96	155·55
Erbium sulphate	Er_23SO_4	620·0	3·678	0·1040	64·48	168·57
Ytterbium sulphate...	Yb_23SO_3	634·0	3·793	0·1039	65·87	167·15
Thorium sulphate	Th_22SO_4	424·0	—	0·0972	41·21	—

The molecular heat of the anhydrous sulphates of the sesquioxides varies only within very narrow limits; 61·60—68·96 for yttrium and didymium sulphate. The values for the salts of chromium, iron, indium, lanthanum, cerium, and didymium are nearly identical; whilst even the values of the remainder approach each other very nearly.

As the following hydrated sulphates are decomposed when heated to 100°, their capacity of heat was determined between 0° and 46° in the vapour of boiling CS_2; the beryllium sulphate not bearing even that temperature without losing water, its molecular heat could not be ascertained.

Hydrated Sulphates.

Compounds.	Formula.	Molecular weight.	Specific weight.	Specific heat.	Molecular heat.	Molecular volume.
Beryllium sulphate.......	$Be_2 3SO_4.12H_2O$	531·3	1·713	—	—	310·17
Yttrium sulphate...	$Y_2 3SO_4.8H_2O$...	611·0	2·540	0·2257	137·91	240·55
Lanthanum sulphate	$La_2 3SO_4.9H_2O$.	728·0	2·853	0·2083	151·64	255·17
Cerium sulphate	$Ce_2 3SO_4.5H_2O$.	657 0	3·220	0·1999	131·33	204·04
Didymium sulphate	$Di_2 3SO_4.8H_2O$.	725·0	2·878	0·1948	141·23	251·91
Erbium sulphate*	$Er_2 3SO_4.8H_2O$.	764·0	3·180	0·1808	138·13	240·25
Ytterbium sulphate†......	$Yb_2 3SO_4.8H_2O$.	778·0	3·286	0·1788	139·11	236·79

On subtracting the values obtained for the anhydrous salts from the same numbers of the hydrated sulphates, we obtain a remainder expressing the molecular heat and volume of the water in combination. Thus the following values are obtained :—

Compounds.	Water in combination.	Molecular heat.	Molecular volume.
Ytterbium sulphate...	$8H_2O$......	9·15	8·70
Erbium sulphate.....	$8H_2O$......	9·21	8·96
Yttrium sulphate.....	$8H_2O$......	9·54	7·72
Didymium sulphate ..	$8H_2O$......	9·03	12·04
Lanthanum sulphate .	$9H_2O$......	9·42	10·88
Cerium sulphate	$5H_2O$......	13·02	11·82

The molecular heat and volume of free water being =18, it will be seen that by entering into combination with the sulphates of the rare earth-metals, its heat and volume are reduced in an extraordinary degree. In fact, the molecular heat and volume of water in these salts descends to a minimum value hitherto unknown.

Comparing the values given above, we readily find that in those groups of compounds intimately connected by isomorphism, the molecular heat of the compounds increases and the molecular volume decreases with increasing molecular weight of the compound or atomic weight of the element. This will be shown by the following table, in which the different isomorphous compounds, partly of yttrium, erbium, and ytterbium, partly of lanthanum and didymium, are brought into comparison :—

* According to Cleve. † According to Nilson.

Elements.	Atomic weight.	Oxides.		Anhydrous sulphates.		Hydrated sulphates.	
		Molecular heat.	Molecular volume.	Molecular heat.	Molecular volume.	Molecular heat.	Molecular volume.
Yttrium	89·5	23·29	44·99	61·60	178·80	137·91	240·55
Erbium	166·0	24·70	43·98	64·48	168·57	138·13	240·25
Ytterbium	173·0	25·45	42·94	65·87	167·15	139·11	236·79
Lanthanum......	139·0	24·42	50·31	66·90	157·22	—	—
Didymium	146·5	27·62	49·07	68·96	155·55	—	—

Especially with regard to beryllia, and the question of its real composition, the values given above are of the greatest importance. In respect of this we observe that: 1st, the atomic heat of oxygen in beryllia is identical with that in alumina and nearly identical with the same value in other closely related oxides, if beryllia $=Be_2O_3$ (see our preceding paper, p. 42); 2nd, under the same supposition the molecular heat and volume of beryllia and alumina are nearly identical, whether the latter earth be examined in the state of crystallised sapphire or of an amorphous powder, and, further, if we consider chrysoberyl not as an aluminate, but as $\left.\begin{matrix}Al_{\frac{4}{3}}\\ Be_{\frac{2}{3}}\end{matrix}\right\} O_3$, this mineral has yielded identical values; 3rd, the molecular heat and value of beryllium sulphate, compared with the same values for the other nearly allied sulphates of aluminium, scandium, gallium, and yttrium, support the formula Be_2O_3 which we have given.

Taking into consideration the above-mentioned circumstances, and those related in our former papers, with the fact that the atomic heat and volume of metallic beryllium, as well as the molecular heat and volume of beryllia and its sulphate, would assume values quite exceptional, if the formula for the oxide were BeO, we think, therefore, the question of the valence of beryllium may be considered as finally decided. In fact, there is no physical property of beryllium, beryllia, or its sulphates, which does not testify to our view being the correct one, and from a chemical point of view the same holds good. It is, we think, unnecessary to take up space here with a repetition of the various reasons which support this conclusion. We refer, therefore, to our detailed paper on beryllium, above quoted, and only mention here that this metal belongs on account of its sulphate,

$$3K_2SO_4 + Be_23SO_4,$$

to the series of the gadolinite and cerite metals, this salt having a composition, typical for all the members of this series (Be, Sc, Y, La, Ce, Di, Tr, Y_α, Y_β, x, Er, Tm, Yb). The series of these elements, the

leading member of which beryllium unquestionably is, stands certainly in the nearest proximity with an other series, that of aluminium (Al_2, Ga_2, Tn_2, Cr_2, Mn_2, Fe_2), but this nevertheless decidedly differs from the former by an other typical double sulphate,

$$K_2SO_4+R_23SO_4+24H_2O.$$

or alum.

The fact, which has been alleged as a proof of the bivalence of beryllium, namely, that the chloride fuses and sublimes at a higher temperature than aluminium chloride is of no importance, for it will be found that amongst the members of the former series, with its difficultly fusible and volatilisable chlorides, many analogies exist for beryllium, not only in this but in many other respects.

Lastly, we subjoin a table showing the magnetic properties of the rare earths. Mr. Knut Ångström kindly undertook this research, employing a powerful electromagnet of Ruhmkorff, between the poles of which the oxides showed the following properties :—

Magnetic.		Diamagnetic.
Cr_2O_3	....	Be_2O_3
Fe_2O_3	....	Al_2O_3
Y_2O_3	....	Sc_2O_3 ?
Di_2O_3	....	In_2O_3
Er_2O_3	....	La_2O_3
Yb_2O_3	....	—
—	....	ZrO_2
CeO_2	....	ThO_2

III. "On the Absorption Spectra of Cobalt Salts." By W. J. Russell, Ph.D., F.R.S., Treas. C.S., Lecturer on Chemistry at the Medical School, St. Bartholomew's Hospital. Received August 4, 1880.

(Abstract.)

The following investigation was commenced with Mr. Lockyer, and although he has been unable to continue the work, the author is indebted to him for much aid and many suggestions.

The cobalt salt first examined was the anhydrous chloride. In order to establish clearly its absorption spectrum, different samples of this salt were made by various processes. All, however, gave the same spectrum. The bromide of cobalt yields a similar spectrum, but its position is different, it is nearer to the red.

On fusing cobalt chloride with potassic chloride, a greenish-blue mass is formed, which gives a spectrum entirely different from that of the chloride when alone. Judging from the fusing-point of the mixture being lower than that of the components, and the cobalt salt not decomposing in this mixture on fusion in contact with air, it seemed natural to suppose that a new compound had been formed, and that it gave rise to the new spectrum. Further experiment showed, however, that this is not the case for other solid chlorides, such as of sodium and zinc give with cobalt chloride the same spectrum; and liquids in which the cobalt chloride easily dissolves, such as ordinary or amylic alcohol, the saline ethers, glycerine, and hydrochloric acid, also give this same spectrum; in fact, this spectrum is produced whenever cobalt chloride dissolves freely in any menstruum without definitely combining with it. A careful set of experiments were made in the case of the solid chlorides, to exclude the presence of water, for it was possible that the spectrum in all the above cases might be due to a trace of water, which, by its combining with the cobalt chloride, formed in every instance the spectrum-giving body.

The bromide and iodide of cobalt, when fused with potassium bromide and potassium iodide respectively, give results corresponding to those of the chloride, but the bands in the spectrum of the bromide, and still more so those of the iodide, are nearer to the red than the corresponding bands of the chloride. The action of heat and of water on the bodies producing these spectra is discussed, and it is pointed out how the definite compound with zinc was indicated by the spectrum. The action of liquids which easily dissolve the cobalt chloride is next described, and as all give the same spectrum, and this spectrum is identical with that obtained with the fused chlorides, the conclusion drawn is, as before stated, that this spectrum must be that of the cobalt chloride, only, owing to solution, it is in a molecular state, different from that obtained on fusing this salt alone. This spectrum, when obtained in the hydrochloric acid solution, is remarkable for its persistence under varying circumstances, and for its being a reaction of great delicacy.

Hydrochloric acid as a solvent for the cobalt chloride differs in one respect from all the other solvents which have as yet been examined, namely, that, whether much or little cobalt be dissolved in it, the spectrum is the same; whereas with dry alcohol, for instance, a saturated, or nearly saturated, solution gives the spectrum above mentioned, but a dilute solution, one containing about 20 grms. of the chloride in 100 cub. centims. of alcohol, gives a spectrum somewhat different: a new band appears and others which were present fade out: if this dilution be carried on still further, so that only about 0·008 grm. of the chloride be present in 100 cub. centims. of alcohol, an entirely different spectrum is obtained, but on carrying the dilution beyond this no

further change takes place. With other liquids which dissolve the cobalt salt freely, a similar series of changes occur, but if liquids in which the chloride is much less soluble be used, then according to their solvent power only the first (or most dilute stage), or the first and second stage, is obtainable; for instance, if dry ether is used as the solvent, it yields only a spectrum corresponding to the first stage. With anhydrous acetic acid, in which the cobalt chloride is more freely soluble, both the first and second stage are obtainable. If the dry chloride in fine powder be shaken up with a liquid in which it is insoluble, such as carbon tetrachloride, then only a spectrum similar to that of the fused chloride is visible.

The anhydrous cobalt chloride dissolved in water gives a pink solution. This solution, when it contains as little as 0·1 grm., or as much as 25 grms., of the salt in 100 cub. centims. of water, gives only a wide absorption-band, shading off on both sides, and whether a short column of the strong solution or a correspondingly long column of the dilute solution be examined, identical spectra are obtained, so that within these limits the same compound appears to exist in the solution. If, however, the solution approaches saturation (100 cub. centims. of water can dissolve at 16° 32 grms. of the cobalt chloride), then another spectrum is visible, and this is again the spectrum of the dissolved chloride: the same spectrum as is obtained either by dissolving cobalt chloride in fused potassium chloride, or in alcohol, or in hydrochloric acid, thus apparently the anhydrous chloride exists in an aqueous solution. The action of heat, and the action of bodies capable of combining with water, in aqueous solutions of cobalt chloride, are identical, both tending to destroy the broad absorption band of the hydrate, and to form the banded spectrum of the dissolved anhydrous chloride.

The very characteristic spectrum of the oxide of cobalt is well known. The precipitate obtained by the addition of potash or soda in excess to any cobaltous salt, shows well this spectrum. If ammonia be the precipitant, a somewhat simpler spectrum is obtained. Vogel has already pointed out the similarity of the spectrum of a piece of cobalt glass and this oxide spectrum. The glass spectrum is apparently similar to the spectrum formed by the precipitate with potash and soda, probably then the extra band visible in these cases and not when ammonia is used, is due to a compound of the alkali and cobalt. The bearing of these spectra on Winkler's supposed cobaltate of potash is then discussed. Further, it is shown that if the above precipitation of oxide be made in solutions in which the cobalt salt is in excess, or even if precipitated oxide be warmed or shaken up in the cold with a solution of cobalt chloride, a new compound is formed, an oxychloride which gives a different spectrum; its formation and its decomposition by water is well traced in the varying spectra producible from it, and

goes hand in hand with the chemical changes which occur. From the spectroscopic appearance it is argued that the blue precipitated oxide is not a hydrate, but that it does very readily undergo change as the mere alteration of colour which takes place shows. Aqueous solutions of the bromide and iodide of cobalt when acted on by alkalies undergo changes similar to those which the chloride undergoes, and, as in the former cases, the iodide spectrum is always nearer the red end of the spectrum than the corresponding bromide spectrum, and the bromide than the chloride spectrum.

The salts of the oxygen acids when in aqueous solution do not give sharp banded spectra as the haloïd salts do, but only a large shading off absorption like the hydrate of the cobalt chloride.

The other points discussed in detail are, first, the nature of the precipitate formed by the action of sodic or potassic carbonate on a cobaltous salt, and it is shown that the formation of the oxide always found in this precipitate is owing to an after decomposition, the precipitate as first formed being entirely free from all oxide, and it gradually appearing after a short time. The other point is the action of heat on cobalt phosphate dissolved in fused microcosmic salt; when cold there appears somewhat indistinctly a banded spectrum of a phosphate, on heating this the spectrum disappears, and the spectrum of the oxide very distinctly takes it place; on cooling, the first spectrum returns, and this change may apparently be repeated any number of times.

Drawings of all the different spectra are given in the full paper.

IV. "On the Friction of Water against Solid Surfaces of Different Degrees of Roughness." By Professor W. C. Unwin, M.I.C.E., Professor of Hydraulic Engineering at the Royal Indian Engineering College. Communicated by J. H. Cotterill, F.R.S., Professor of Applied Mechanics, Royal Naval College, Greenwich. Received August 31, 1880.

(Abstract.)

These experiments relate to the friction of fluids when flowing against rough solid surfaces. It is well known that a board dragged through water suffers a resistance which, at speeds not very small, varies nearly as the square of the velocity. The fluid surrounding the board does not behave as a solid, but shearing and eddying motions are set up which give rise to losses of energy distributed throughout the fluid mass.

Most of the existing knowledge of fluid friction has been derived from the observation of the flow of water in pipes and canals. But in

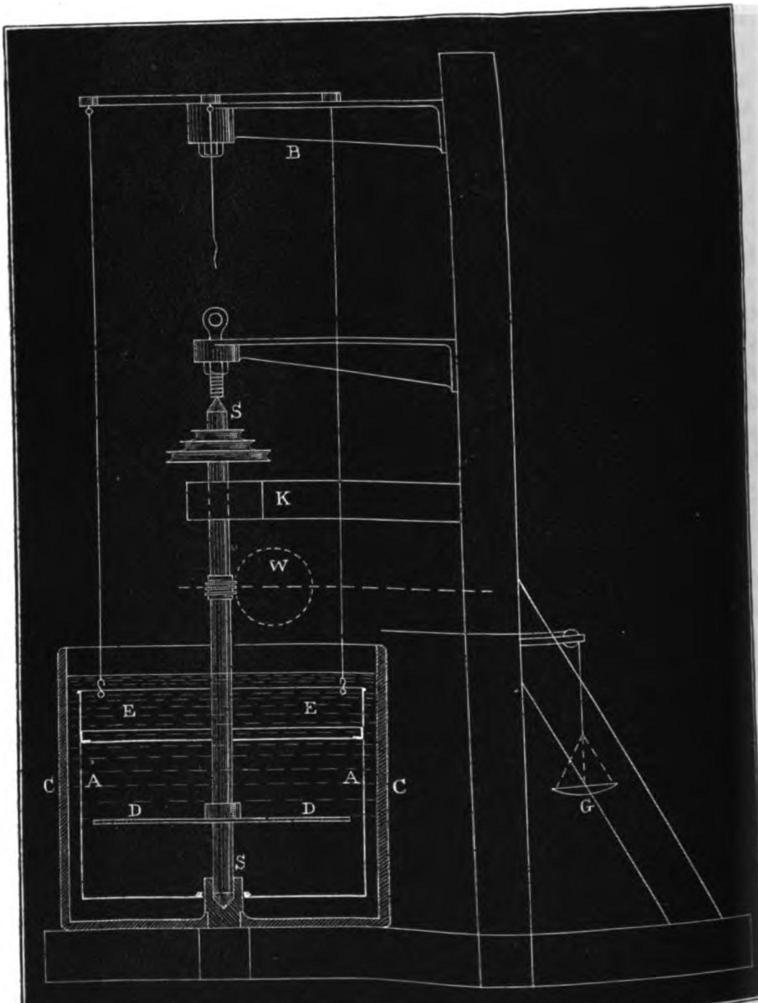
these cases, the motion of the fluid is complex, and the observations themselves are difficult. The principal direct experiments on fluid friction are those of Coulomb and of the late Mr. W. Froude. Coulomb's experiments were made by oscillating a thin disk, suspended in the fluid by a wire, in its own plane. The gradual diminution of the range of the oscillations gave a measure of work lost in fluid friction, on the surface of the disk. Coulomb's experiments were made at very low velocities, and, indeed, the method which he employed would be quite unsuitable for greater speeds of oscillation. The results at which he arrived were these:—(*a.*) The frictional resistance to the motion of the disk varied nearly as the velocity; (*b.*) The friction was nearly independent of the roughness of the surface; (*c.*) The friction was very much increased if the viscidity of the fluid was increased.

Mr. Froude's experiments were made in a very different way. He towed boards of lengths varying from 5 to 50 feet, in a still water canal, and measured by a spring dynamometer the resistance to the motion. His experiments were all made at speeds much higher than those employed by Coulomb. His results may be summarised thus:—(*a.*) The friction varies nearly as the square of the velocity of the board, the precise index of the speed to which the friction is proportional depending on the nature of the surface of the board; (*b.*) The frictional resistance per square foot of the surface of the board is greater for short than for long boards; (*c.*) The frictional resistance varies very greatly with the roughness of the surface of the board.

The differences between the results of the experiments of Coulomb and Froude show that the phenomena of fluid friction at very low and at high speeds are essentially different. It appeared to the author that it would be useful to make some experiments at speeds similar to those in Mr. Froude's experiments, but with an apparatus on a smaller scale, which would permit a greater variation of the conditions of the experiments. A series of disks, of 10 inches to 20 inches in diameter, were rotated in water by an engine, and the resistance to continuous rotation at different speeds was measured. Thus the experiments were virtually the same as Mr. Froude's, but with a surface of infinite length substituted for surfaces of limited length. The roughness of the surface of the disk was varied, the smoothest disks being of turned and polished brass; the roughest having surfaces of sand and gravel cemented on to the metal disk.

The disks were rotated in a cylindrical chamber, the size of which could be varied, so that the mass of water operated on, or the thickness of the layer of water in contact with the disk, could be modified at will. Further, the roughness of the surface of the chamber in which the disks were rotated was varied in the same way as the roughness of the disks themselves. To determine the effect of the viscidity of the liquid in altering the amount of friction, experiments were made with

a solution of sugar in water. Lastly, a series of experiments were made with water at different temperatures. Altogether several hundred observations of the resistance of the disks under different conditions were recorded.



The figure shows a section of the apparatus employed. DD is the disk, the friction of which, when rotated in fluid, is to be measured. This is keyed on a shaft, SS, which was driven from a hot-air engine by means of a catgut belt. The disk is placed in a cast-iron cistern, CC,

containing the fluid. Between the disk C and the outer cistern, however, is the light cylindrical copper chamber, AA, suspended by three fine wires from a crosshead, B. EE is a diaphragm, which could be moved up or down so as to alter the volume of water in the chamber in which the disk rotates. K is a brake for regulating the speed of the disk. W is the position of a counting arrangement for determining the speed of the disk. G is a scale-pan in which weights were placed. This was connected by a fine silk cord with an arc attached to the chamber AA. The weight in this scale-pan exactly measures the tendency of the chamber AA to rotate, in consequence of the rotation of the fluid inside it. But since action and reaction are equal, the tendency of the chamber AA to rotate is, when the motion is uniform, exactly equal to the friction of the surface of the disk, DD. This method of measuring the friction of the disk, by measuring the reaction of the vessel containing it, was first used (so far as the author is aware) by Professor James Thomson. A short note on some experiments made in this way was communicated by him to the Royal Society in 1855, but the details of these experiments have never been published. The principle is the same as that employed by Mr. Froude, in his "Fluid Dynamometer." The general results of the experiments made with the apparatus described above are in striking numerical agreement with Mr. Froude's results, so far as the conditions of the experiments are similar. But, from the small size of the apparatus, it has been possible to vary the conditions somewhat more than would be possible with the great canal of Mr. Froude. The results obtained may be summarised as follows:—

(1.) The resistance of disks of different diameters, but similar in other respects, varies as the fifth power of the diameter nearly, or, more exactly, as the 4·85th power.

(2.) The resistance varies with the roughness of the surface of the disk to an extent quite as great as in the experiments of Mr. Froude.

(3.) The friction *increases* in every case with the size of the chamber in which the disk is rotated. This result was certainly unexpected. Even if the increase of resistance is due to the increase of the surface of the chamber, this result indicates another marked difference between the phenomena of fluid friction at high and at low speeds. At very low speeds the resistance would decrease considerably as the size of the chamber increased.

(4.) Roughening the surface of the chamber in which the disk is rotated increases the resistance of the disk considerably; in some cases the increase is as great as when the disk itself is roughened.

(5.) The resistance of the disk at different speeds varies nearly as the square of the speed. But the exact power of the speed to which the resistance is proportional varies a little for different surfaces. The indices of the powers of the speed to which the resistance is pro-

portional are almost exactly the same for similar surfaces, in these experiments, as in Mr. Froude's experiments.

(6.) A series of experiments were made on the influence of the temperature of the water on the friction, and the author is not aware that any direct experiments on fluid friction, at different temperatures, have previously been made. The experiments show that the friction diminishes rapidly with increase of temperature. The alteration is so great that even five degrees variation of temperature alters the friction by about one per cent.

(7.) When the viscidity of the fluid was increased by dissolving half a hundredweight of sugar in the water of the cistern, the frictional resistance of the disk was increased. But the proportionate increase of resistance was much less than that observed by Coulomb, in a similar experiment at a very low velocity.

At the close of the Meeting Professor Graham Bell made experiments with his Photophone.

November 25, 1880.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election was read, as follows:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D., LL.D.

Other Members of the Council.—William Henry Barlow, Pres. Inst. C.E.; Rev. Professor Thomas George Bonney, M.A., Sec. G.S.; George Busk, F.L.S.; Right Hon. Sir Richard Assheton Cross, G.C.B.; Edward Dunkin, V.P.R.A.S.; Alexander John Ellis, B.A.; Thomas Archer Hirst, Ph.D.; William Huggins, D.C.L., LL.D.; Professor John Marshall, F.R.C.S.; Professor Daniel Oliver, F.L.S.; Professor Alfred Newton, M.A., Pres. C.P.S.; Professor William Odling, M.B., V.P.C.S.; Henry Tibbats Stainton, F.G.S.; Sir James Paget, Bart., D.C.L.; William Henry Perkin, Sec. C.S.; Lieut.-Gen. Richard Strachey, R.E., C.S.I.

Mr. A. J. B. Beresford-Hope was admitted into the Society.

The Right Hon. Sir G. Jessel, Knt., whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Chemical Composition of Aleurone-Grains." By S. H. VINES, M.A., D.Sc., Fellow of Christ's College, Cambridge. Communicated by Dr. MICHAEL FOSTER, F.R.S., Prælector of Physiology in Trinity College, Cambridge. Received September 17, 1880.

The following is an account of further researches on this subject; abstracts of results have already been given in "Proc. Roy. Soc.," vol. 28, p. 218, and vol. 30, p. 387.

IV. *The Aleurone-Grains of the Sunflower* (Helianthus annuus).

a. Microscopical Observations.—The sections of the seeds were treated with ether or alcohol, to remove the oil.

The grains became vacuolated on treatment with water.

They dissolve completely in 10 per cent. NaCl solution.

If they have been previously treated with alcohol, they dissolve readily and completely in saturated NaCl solution; but if they have been previously treated with ether they only become vacuolated.

b. Chemical Observations.—The seeds were ground in a hand-mill, and treated with alcohol or ether to remove the oil.

The watery extract of the seeds gives no precipitate on boiling. On concentrating the fluid, and then allowing it to filter into alcohol, a dense precipitate is formed. This substance is readily soluble in distilled water, and its solution gives the xanthoproteic and Millon's reactions, a rose colour with KHO and $CuSO_4$, a precipitate on the addition of HNO_3, and an immediate precipitate on the addition of potassic ferrocyanide after acidification with acetic acid.

The 10 per cent. NaCl extract gives a precipitate on boiling, and on saturation with NaCl.

The saturated NaCl extract gives, when the seeds have previously been treated with alcohol, a dense precipitate on boiling, and on dilution; if the seeds have been previously treated with ether, the amount of the precipitate is much less; boiling produces little more than a turbidity.

These observations, taken together, show that these grains contain: (1) a substance (vegetable peptone or hemialbumose) which is soluble in water; (2) a substance which is soluble in 10 per cent. NaCl solution, precipitable from its solution by saturation with NaCl, and which therefore belongs to the group of myosin-globulins; (3) a substance which is soluble in saturated NaCl solution, whether the grains have been treated with alcohol or ether, and which therefore belongs to the group of the vitellin-globulins; and (4) a substance which, like the crystalloids of Ricinus described in a previous communication, is soluble in saturated NaCl solution only after previous treatment with alcohol.

V. *The Aleurone-Grains of the Brazil-Nut* (Bertholletia excelsa).

a. Microscopical Observations.—Like those of Ricinus, the grains of this plant present no indication of a complex structure when mounted in alcohol; on the addition of water they become transparent, and the crystalloid, as well as the curiously irregular globoid, can be seen.

On treatment with 10 per cent. NaCl solution, the whole grain (excepting, of course, the globoid) dissolves.

Treatment with saturated NaCl solution produces the same result,

and it is not affected by previous treatment of the grains with ether or alcohol.

b. Chemical Observations.—The seeds were crushed in a mortar, and then treated with either alcohol or ether to remove the oil.

The watery extract of the seeds gives a slight precipitate on boiling; the filtrate gives no precipitate on boiling; it gives the reactions of a fluid holding peptones in solution; it also gives a precipitate with HNO_3.

The 10 per cent. NaCl extract gives a dense precipitate on boiling, as well as on dilution, and on saturation with NaCl.

The saturated NaCl extract gives a dense precipitate on boiling and on dilution.

From these observations it appears that these grains consist of vegetable peptone and of globulins, the one belonging to the myosin, the other to the vitellin group.

Weyl has shown ("Zeitschr. f. Physiol. Chem.," Bd. I, 1877) that the crystalloids of these grains consist of pure vitellin; hence the peptone and the myosin must be contained in the ground-substance of the grains.

General Remarks.

The investigation of the aleurone-grains of a number of different species of plants has shown that, with a few exceptions mentioned below, they may be classified under the five types which have been described in this and previous communications. I find, contrary to the opinion of Pfeffer ("Jahrb. f. wiss. Bot." viii, 1872), that all the aleurone-grains which I have examined are soluble to some extent at least in water; they are also all soluble to some extent in 10 per cent. NaCl solution. In the cases which have been described in detail, the grains were found to be completely soluble in this solution, but in others I found that the grains were only partially soluble in it, residue dissolving either in 1 per cent. Na_2CO_3 solution, or in dilute KHO, and therefore consisting of some form of albuminate, which may be regarded as altered globulin.

The proteid substances detected in the grains may be classified as follows:—

I. *Soluble in distilled water:*—

Vegetable peptone (hemialbumose?).

II. *Insoluble in distilled water:*—

a. Soluble in 10 per cent. NaCl solution. Globulins.

(*α.*) *Insoluble in saturated* NaCl *solution,*—

Vegetable myosin.

(*β.*) *Soluble in saturated* NaCl *solution, after treatment with alcohol,*—

Substance of crystalloids of Ricinus, &c.

(γ.) *Soluble in saturated* NaCl *solution after ether or alcohol,*—Vegetable vitellin.

b. Insoluble in 10 per cent. NaCl solution. Albuminates.

(α.) *Soluble in* 1 *per cent.* Na_2CO_3 *solution.*

(β.) *Soluble in dilute* KHO.

I have placed by itself the peculiar proteid which constitutes the crystalloids of Ricinus, and which occurs in the grains of Helianthus, for, although, as I have previously pointed out, it resembles myosin in its properties before treatment with alcohol and vitellin after it, it differs from both these substances in that it is less readily soluble in 10 per cent. NaCl solution.

The following is an arrangement of the species examined according to the solubility of the grains. It must be borne in mind, however, that the observations upon which this arrangement depends are, for the most part, simply microscopical, but the close agreement between the results of microchemical and macrochemical methods in the cases which have been given at length justifies an inference as to the probable composition of a grain from the results of one method only.

Classification of Aleurone-Grains (Microchemical).

I. *Soluble in water:*—

Pæonia officinalis (type). Ranunculus acris. Aconitum Napellus. Anemone Pulsatilla. Nigella damascena. Helleborus fœtidus. Amygdalus communis. Prunus cerasus. Pyrus malus. Cynara Scolymus. Scorzonera hispanica. Leontodon Taraxacum. Dipsacus Fullonum. Ipomœa purpurea. Phlox Drummondi. Fœniculum officinale. Impatiens glandulifera. Vitis vinifera.

II. *Completely, and more or less readily, soluble in* 10 *per cent.* NaCl *solution.*

a. Grains without crystalloids.

(α.) *Soluble in saturated* NaCl *solution after treatment with alcohol or ether:*—

Lupinus hirsutus (type). Vicia Faba. Pisum sativum. Phaseolus multiflorus. Allium Cepa. Iris pumila (*var.* atrocœrulea). Colchicum autumnale. Berberis vulgaris. Althæa rosea. Tropæolum majus. Mercurialis annua. Empetrum nigrum. Primula officinalis.

(β.) *Soluble in saturated* NaCl *solution after alcohol, but not after ether:*—

Helianthus annuus (type). Platycodon (Wahlenbergia) grandiflora. Erodium gruinum. Sabal Adansoni. Delphinium car-

diopetalum. Trollius europæa. Actea spicata. Caltha palustris. Aquilegia vulgaris. Campanula rotundifolia. Dianthus Caryophyllus. Brassica rapa. Lepidium sativum. Medicago sativa, Cedrus Deodara. Larix europæa. Ephedra altissima. Cynoglossum officinale. Spinacia oleracea.

b. Grains with crystalloids.

(*a.*) *Crystalloids soluble in saturated* NaCl *solution after treatment with alcohol or ether* :—

Bertholletia excelsa (type). Adonis autumnalis. Æthusa Cynapium. Digitalis purpurea. Cucurbita Pepo.

(*β.*) *Crystalloids soluble in saturated* NaCl *solution after alcohol, but not after ether* :—

Ricinus communis (type). Datura Stramonium. Atropa Belladonna. Elaïs guineensis. Salvia officinalis. Taxus baccata. Pinus Pinea. Cannabis sativa. Linum usitatissimum. Viola elatior. Ruta graveolens. Juglans regia.

III. *Partially soluble in* 10 *per cent.* NaCl *solution.*

a. Entirely soluble in 1 per cent. Na_2CO_3 solution :—

Pulmonaria mollis. Omphalodes longiflora. Borago caucasica. Myosotis palustris. Clarkia pulchella.

b. Entirely soluble in dilute KHO.

(*a.*) *Grains without crystalloids* :—

Anchusa officinalis. Lithospermum officinale. Echium vulgare. Heliotropium peruvianum. Lythrum Salicaria.

(*β.*) *Grains with crystalloids* :—

Cupressus Lawsoniana. Juniperus communis. Euphorbia Lathyris.

II. "On the Ossification of the Terminal Phalanges of the Digits." By F. A. DIXEY, B.A. Oxon. Communicated by E. A. SCHAFER, F.R.S. Received October 5, 1880.

[PLATES 1, 2.]

From the Physiological Laboratory of University College, London.

In a preliminary note on the ossification of the terminal phalanges of the digits,* it was stated that the diaphyses of the ungual phalanges differed in their mode of ossification from those of other long bones. The object of the present paper is to give an account of the process of

* *Ante*, vol. 30, p. 550.

ossification in these phalanges, the peculiar features of which have been found, with slight modifications, to remain constant throughout the whole series of Vertebrata.

It will be remembered that all long bones are at an early stage preformed in cartilage, and that simultaneously with the calcification of the cartilaginous shaft a deposit of true bone takes place in the investing subperiosteal tissue; so that at a certain stage in development the so-called "primary" or cartilage bone of the shaft is surrounded by a hollow cylinder of "secondary" or true bone. An irruption of the osteoblastic subperiosteal tissue into the cartilage then takes place, which results eventually in the absorption of the primary bone and its replacement by true osseous tissue, to be in its turn alternately absorbed and renewed so long as the process of growth goes on.

Cartilage that is about to undergo calcification presents certain characteristic appearances; the cells with their cell-spaces become larger, flatten out, and usually show a tendency to arrange themselves in parallel rows, between which, if the change has already been in progress for some time, the lines of calcification may be seen advancing. But whereas in the long bones as a whole,* including the first and second phalanges of the digits, the alteration of the cartilage cells, followed by calcification of the matrix, appears first in the centre of the shaft and spreads thence *pari passu* towards the two extremities; in the ungual phalanx it is first seen to arise in the tip or distal extremity of the cartilage, from which point it spreads gradually backwards towards the base of the phalanx (figs. 2, 3). Similarly the subperiosteal deposit of membrane bone in all other cases begins as a thin and narrow ring surrounding the shaft and placed midway between the two extremities, that is to say, in direct relation with the spot where the cartilage first begins to calcify; in the ungual phalanx, however, still preserving its relation with the point of commencing calcification in the cartilage, the deposit of subperiosteal bone first appears as a thin layer closely applied to the tip of the cartilage and fitting over it like a cap. The ring or hollow cylinder of bone formed in ordinary cases enlarges in two ways, becoming thickened by the continual deposition of new osseous tissue on its outer surface, and at the same time growing at its edges towards the extremities of the shaft, thus accompanying along the outer surface of the cartilaginous diaphysis

* Dec. 9.—Further observations have shown that the received account as given above requires some modification. In the ordinary phalanges, which differ from most other long bones by having only one epiphysis, the changes accompanying the calcification of the cartilage do *not* go on in precisely the same manner towards each extremity. The well-known lines of advancing calcification which run between rows of single cells parallel with the axis of the shaft, are in the ossifying phalanx only visible towards the proximal end; that is to say, the end which will be crowned by the future epiphysis. As might be expected, there is no difference in this respect between the proximal ends of the last and of the other phalanges.

the changes that are taking place within; the whole process resulting in the assumption by the bony cylinder of the form of a dice-box with both ends open, and the median constriction partially or entirely filled in from the outside. Subject to its peculiar conformation, the growth of the bony layer in the ungual phalanx is analogous to the foregoing process. A thickening takes place by the addition of new bone to the outer surface of the cap, and simultaneously with this its edges grow backwards along the cartilage from the tip towards the base (in the same direction as that previously taken by the process of cartilage calcification), so that the cap becomes deeper and deeper, and finally reaches a stage in which it may be compared to a thimble fitting over the cartilaginous phalanx and enclosing it almost up to its base (see figs. 1, 2, 3, 4, *p*, *p*). The deposition of new bone along the outer surface of this cap or thimble may take place relatively much more quickly at the summit than at the sides, so that an expansion is formed, which may be of considerable size, and which no doubt bears a relation to the conformation of the future nail, hoof, or claw (see fig. 2). The next stage in the development of the bone is marked by the irruption of the subperiosteal tissue with blood-vessels and osteoblasts into the shaft at or near the point where the cartilage first began to calcify. This point in all other cases will be found about the middle of the shaft, but in the ungual phalanx, as has been seen, at the tip. In fig. 1, which represents a section in the sagittal plane through the growing ungual phalanx of a fœtal cat, the irruption is seen to have taken place at a point just below the tip on its plantar aspect, and this would appear to be the most usual locality. In figs. 2, 3, representing similar sections from the pig and the human subject at an earlier stage of development, the points at which the invasion will probably begin are indicated by the letters *i*, *i*. The advance of the osteoblastic tissue into the cartilaginous diaphysis, its gradual absorption of the primary bone, and the laying down of true osseous tissue in its stead, follow in all cases the direction already taken by the previous process of calcification; that is to say, in other long bones from the middle towards the two extremities, in the ungual phalanx from the tip towards the base. Thus, to sum up, in the development of the ungual phalanx the three processes of cartilage calcification, growth of the subperiosteal intramembranous bone, and deposition of true bone in the shaft along the line of advance of the osteoblastic ingrowth, take the distal extremity of the shaft instead of its middle for their starting-point, and proceed in one uniform direction from tip to base, instead of advancing in two contrary directions at the same time. Hence it seems that the distal extremity of the ungual phalanx corresponds morphologically with the centre of the diaphysis in other long bones.

At a period of growth subsequent to the complete ossification of the diaphysis, an epiphysis forms, as is well known, in the cartilaginous

head which still remains at the proximal end of the phalanx, and this becomes united to the shaft in the usual manner.

The process of ossification in the terminal phalanx having now been described in general terms, it only remains to notice certain special points of interest that have come under observation during the investigation of the subject.

It should be first mentioned that the peculiarity described appears to be universal, the same general description applying to any terminal phalanx, whether taken from the manus or pes, and whether belonging to a fully functional or to a mere aborted digit, such as the second and fifth in the manus of the pig. An examination of the growing bone with a view to this point would probably suffice to decide whether the phalanx missing from certain digits in the manus of Pteropus and other bats is really the third (as described, see Flower, "Osteology of the Mammalia," 1876, p. 264), or whether it may not rather be the second.

A comparison of a series of specimens taken from man, the mole, the rat, the pig, and other animals, seems to indicate that the primitive cartilaginous terminal phalanx exists normally as a subcylindrical bar of comparatively simple conformation;* while the remarkable modifications of shape which the adult phalanx assumes throughout the whole range of the mammalia are due to the superstructure erected on this basis by purely intramembranous ossification in the form of the subperiosteal cap, and of what may be called the ungual expansion at its summit. Thus, in the mole, the primitive cartilaginous terminal phalanx presents no distinctive character; it resembles in shape the cartilage of the young pig, as seen in fig. 3, but is relatively a little shorter and thicker. When, however, the subperiosteal growth begins, it is seen that a separate centre for the accumulation of the bony deposit has established itself on each side of the tip of the phalanx, the two being connected by a film of bone over the tip itself, so that the resulting cap has two summits instead of one. At a later stage these two summits are found to have increased considerably in height and thickness, and to have encroached upon and partly filled up the interval between them. In this way the growth of the bifid cap proceeds until, after the normal changes, the ungual phalanx of the manus in the adult mole presents a highly modified appearance, being unusually long and deeply cleft at its extremity, thus contrasting strongly with the cartilaginous basework on which it was fashioned. Again, in the cat and other carnivora, the hood-like expansion of bone which in the adult is reflected over the base of the claw must owe its origin entirely to a process of intramembranous ossification, since no trace

* This shape is liable to a slight modification from the growth of the cartilage at its proximal end after the process of calcification has already begun at the tip. See description of fig. 2.

of it is discoverable in the specimen drawn in fig. 1, in which the cartilaginous diaphysis has received its final development, and is already in process of being destroyed. That this structure makes its first appearance at a very late stage in the development of the phalanx, is shown by the fact that it had not yet begun to be formed in another cat embryo 8 centims. long. An examination of the process of growth in the long tapering ungual phalanges of certain seals might show that for the greater portion of their length they were formed entirely from the ungual expansion of the subperiosteal cap, and had never pre-existed as cartilage. Illustrations of the simple ontline of the primitive cartilaginous diaphysis, and of the variety of the forms that may be assumed by its bony superstructure, are afforded by figs. 2, 3, in neither of which has any absorption of the cartilage yet taken place. In fig. 3, taken from a fœtal pig, the young phalanx, which is destined to support a hoof, is seen to have its subperiosteal cap greatly enlarged on the palmar aspect.

As an example of the process in birds, the terminal phalanges of the second and third digits of the manus, and of all the digits of the pes, were examined in a young sparrow. The calcification of the cartilage and its attendant changes, and the formation of the subperiosteal cap, were seen to be starting from the tip, and proceeding in exactly the same manner as already described; specimens, however, to show the invasion of the cartilage by osteoblastic tissue could not at that time be conveniently obtained.

Among reptiles, a young alligator showed the same processes with great clearness; but it was again impossible to procure a specimen sufficiently advanced to exhibit the course of the osteoblastic irruption.

An interesting modification of the process is furnished by the amphibia. Fig. 4 represents a section in the sagittal plane through the last two phalanges of the middle digit of the fore-foot of the Proteus. In this specimen, which there was every reason to suppose adult, no irruption whatever of osteoblastic tissue has taken place in the last two phalanges, which accordingly consist each of a core of partly calcified cartilage; in the case of the penultimate phalanx surrounded by the hollow cylinder or dice-box of true bone already alluded to, in the case of the terminal phalanx plunged into a deep receptacle, also of true bone, which is in fact the subperiosteal cap.* Thus a state of things which in the higher Vertebrata belongs only to a temporary stage in embryonic development, is in the Proteus persistent throughout life. The growth and calcification of the cartilage and the formation of the subperiosteal layer of bone proceed, it may be assumed, in the regular manner; but having advanced so far the process stops,

* From the only specimens procurable, no conclusions could be arrived at with regard to the condition of the proximal phalanx of the digit, or indeed of any other of the bones.

and the final stages marked by the invasion of the calcified cartilage and its replacement by true bone are in these two phalanges never reached at all.* [*Note.*—Another instance of arrested development in the digit of the Proteus is afforded by the interphalangeal joint represented in the same figure. The cartilage, with a slight alteration in the size and relative number of its cells, is seen to be quite continuous between the heads of the two phalanges, nor does it exhibit the least sign of an articular cavity. The movement allowed by such a joint must be limited, but that some does take place is rendered probable by the position of the tendons seen at *et* and *ft*. Specimens of intramembranous ossification in the Proteus are remarkable as showing with great distinctness the original deposition of bone in the form of globules, after a manner strictly comparable with the growth of dentine.] In the newt the ossification of the terminal phalanx proceeds in the same way as in the Proteus, and is arrested at the same stage; but the penultimate phalanx undergoes the osteoblastic invasion, and is remodelled in true bone in the usual way.†

The specimens from which the figures and descriptions were taken were all hardened in strong spirit, most of them having been previously decalcified in weak chromic or saturated picric acid. They were then imbedded by the cacao-butter method and cut by hand or by Leiser's microtome. The greater number were stained as sections with magenta, but some were stained in bulk with logwood, carmine, or both, before being subjected to the process of imbedding. The best results were obtained by the employment of a freshly-prepared solution of magenta in oil of cloves.

DESCRIPTION OF PLATES.

Figure 1. Section in the sagittal plane through the terminal phalanx of a digit in the pes of a fœtal cat 4 centims. long. Decalcified in chromic acid and stained with magenta. *c*, the unaltered cartilage of the base of the phalanx; towards the tip the cells are seen to be assuming the form and

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* This condition resembles one that is liable to occur in the costal cartilages of the mammalia, where a layer of membrane bone (in these cases subperichondrial) is found enveloping a core of cartilage which may or may not be calcified.

† Dec. 9.—The uncertainty that prevails respecting the homologues of the limb-elements in fishes, renders it difficult to say how far this class can be brought under the rule above laid down for the higher vertebrates. In connexion with this subject, which is still under investigation, the following facts are noticeable:—(1.) That in the limbs of all fishes which possess these appendages there is found at an early stage a cartilaginous endoskeleton, which may persist as such, or undergo various degrees of subsequent ossification. (2.) That to this set of elements is superadded in many fishes a series of dermal deposits of bone (with or without lacunæ), the local relation of which to the endoskeletal elements corresponds with that of the subperiosteal bony growth to the terminal cartilaginous phalanx in the higher vertebrates.

arrangement characteristic of approaching calcification; at *cc* the calcification is complete. *a* marks the furthest point yet reached by the osteoblastic invasion, where secondary areolæ containing numerous osteoblasts have been formed by the partial demolition of the calcified cartilage; and at *i* the ingrowth of the subperiosteal tissue with blood-vessels and osteoblasts is seen to have begun close to the tip on its plantar aspect, and to be spreading back towards the base of the phalanx, having effected the total absorption of the primary bone in the neighbourhood of the point of invasion; *p*, the subperiosteal cap of intramembranous bone, at this stage thin and incomplete; osteogenic fibres and osteoblasts are seen along its outer border, and lacunæ in its substance. *pp*, layer of bone marking the former limit of the subperiosteal cap, which has suffered to a great extent from absorption, and is being remodelled on a larger scale. *ft*, *et*, flexor and extensor tendons, some fibrous bundles from which are seen to be prolonged into the growing bone. *bv*, blood-vessels in the osteoblastic tissue.

[Magnified about 85 diameters.]

Figure 2. Sagittal section through the terminal phlanax of the fifth digit of the right manus of a human fœtus about the fourth month. Decalcified in picric acid, stained with magenta. *c*, unaltered cartilage; *cc*, calcified cartilage. At *s*, a region occurs where the cartilage presents unusual characteristics, the cell-spaces being smaller and the matrix more deeply stained than in the areas on either side, which show the more typical appearance of primary bone. *p*, the subperiosteal cap of intramembranous bone, with osteogenic fibres and osteoblasts. Many fibrous bundles, continuous with the tendons *et* and *ft*, are seen as in the last figure to be prolonged into the bone. The ungual expansion at the summit of the cap consists of two masses (*d* and *pa*), separated by a notch, *i*, which may represent the point of future irruption. A series of sections from the same digit shows that what appears here as a notch is really a pit sunk in the middle of the ungual expansion. Of the two masses (*d* and *pa*), the one towards the dorsal surface (*d*) is characterised by possessing in proportion far fewer lacunæ with their included bone-cells than the protuberance (*pa*) on the palmar side. This distinction applies also to the dorsal and palmar sides of the cap as far as they extend. In this specimen no irruption has yet taken place, and the cartilage preserves its original form. The conical contour indicated by the figure is due to the fact that expansion can only take place in cartilage as yet untouched by the process of ossification, its growth when this begins being checked either by the calcification of its own matrix or by the obstacle opposed to it by the investing subperiosteal layer of bone. Hence, only the proximal area of the cartilaginous phalanx is capable of lateral expansion, and since the growth of the cartilage continues for a long time after the beginning of the process of ossification at its tip, the conical shape seen in this and other instances results. The same cause produces the well-known hour-glass form of the cartilage seen during the growth of other long bones.

[Magnified 65 diameters.]

Figure 3. Sagittal section through the terminal phalanx of one of the middle digits of the manus of a fœtal pig 6 centims. long. Decalcified in picric acid, stained with magenta. *c*, unaltered cartilage; *cc*, calcified cartilage. *p*, subperiosteal cap of bone, which is greatly thickened on the palmar aspect of the phalanx. A deep cleft is seen separating the greater bulk

of this part of the cap from a thin layer of bone, *pp*, which is closely adherent to the cartilage; in the interval a large blood-vessel is seen cut across, and a smaller branch runs up towards *i*, which is near the point where the osteoblastic irruption will in all probability begin. Other blood-vessels are seen at *bv*. This section represents very nearly the same stage in ossification as fig. 2.

[Magnified about 85 diameters.]

Figure 4. Sagittal section through the last two phalanges of the middle digit of the manus of *Proteus anguinus*. *c*, unaltered cartilage; *cc*, cartilage calcified, but exhibiting no other change. In the terminal phalanx the distal extremity of the cartilaginous core is seen to be narrowed down to a single row of cells with the intervening matrix. *p*, subperiosteal cap of bone. *b*, cylinder or dice-box of bone enveloping the shaft of the penultimate phalanx. *l*, *l*, lacunæ. *j*, interphalangeal joint. *et*, *ft*, extensor and flexor tendons. This specimen was stained with magenta, but not decalcified, for which reason the bony tissue appears light and transparent, instead of being deeply stained, as in the preceding figures. The permanently calcified cartilage shows no alteration in the arrangement or size of the cells, which in this instance are remarkably large and regular in form. The animal was believed to be fully adult.

[Magnified about 85 diameters.]

(Received October 27, 1880.)

The fact that the process of ossification in the terminal phalanges begins at the distal extremity, and advances towards the base, has been observed as regards the human fœtus, by M.M. Rambaud and Renault ("Origine et Développement des Os," Paris, 1864). After describing the order in which the first and second rows of phalanges are ossified in the hand, they continue, "De même pour les troisièmes, où elle (*i.e.*, l'ossification) débute par le rebord unguéal" (p. 214). In speaking of the foot, their language is more explicit, "Les troisièmes phalanges commencent à s'ossifier par l'extrémité antérieure, puis de là l'ossification s'étend vers l'extrémité postérieure, laissant toujours un appendice cartilagineux qui se transforme en épiphyse" (p. 240). These authors, however, make no attempt to describe the process, nor to determine how far it is participated in by the cartilage and the superiosteal membrane respectively; neither do they mention the ungual expansion, or the enveloping cap. Moreover, the statements already quoted from the text of their work are entirely ignored in the plates, where in every figure that contains a representation of growing phalanges, and even in the figures especially referred to as illustrative of the above-quoted statements, the terminal phalanx is made to exhibit no difference whatever from the rest in its mode of ossification. For instance, in Plate XXI, fig. 1, it is drawn with a very evident cartilaginous head at each extremity; yet this figure, in the words of the authors themselves, "représente exactement l'état de l'ossification de la main à la naissance." (*Ibid.*, p. 215.)

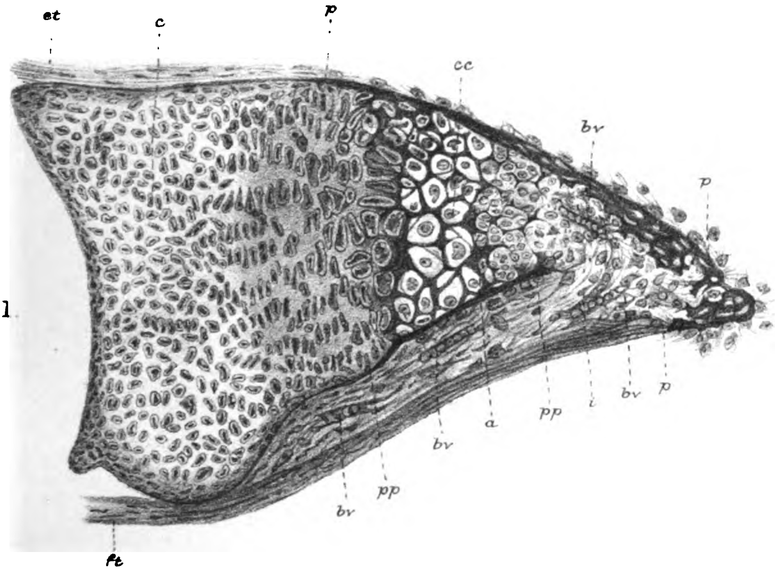


Fig 1.

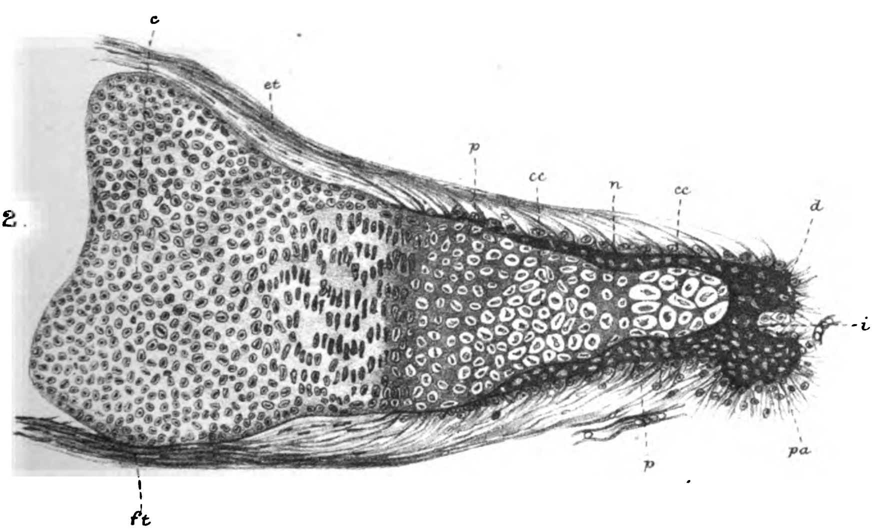
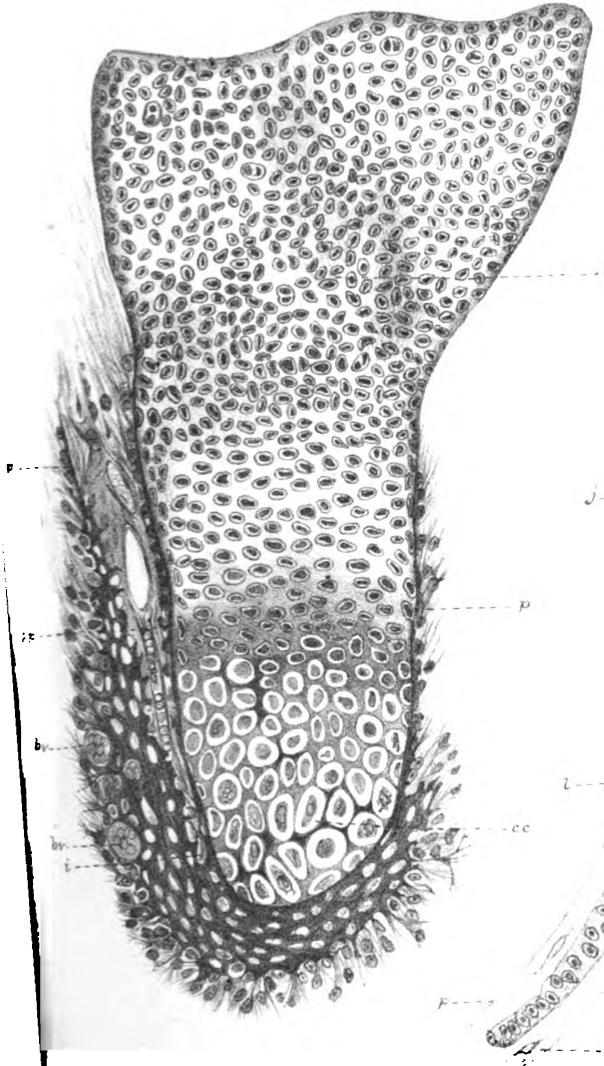


Fig. 2.

West, Newman & Co. lith.

Fig. 3.



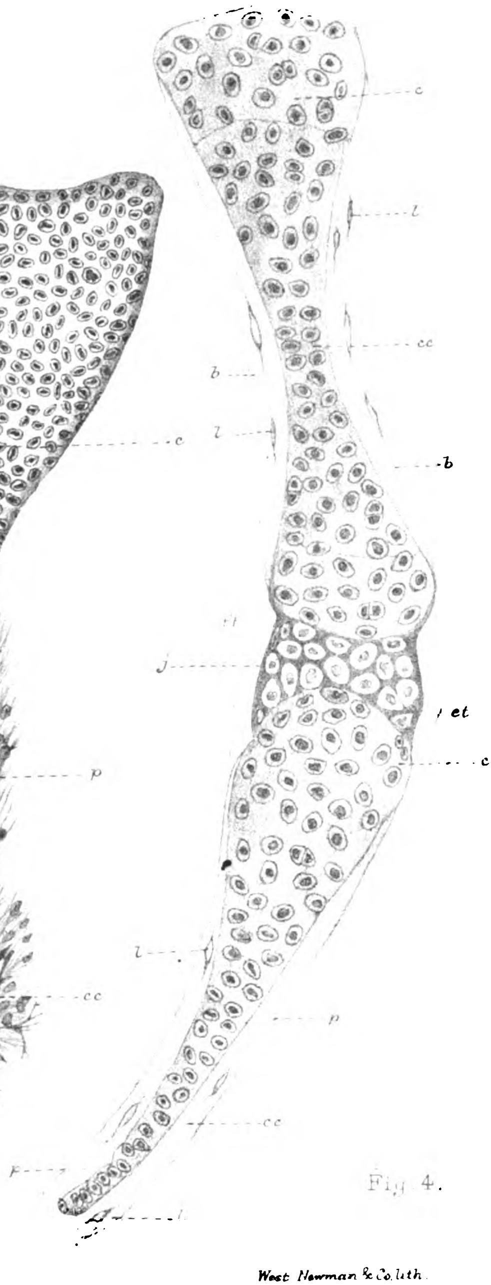


Fig. 4.

West Newman & Co. lith.

A recent paper by Dr. M. Kassowitz ("Die Normale Ossification, etc.," in S. Stricker's "Medizinische Jahrbücher," 1880), is accompanied by a plate (Taf. X), in which there is diagrammatically represented a sagittal section through a finger of a human fœtus of slightly earlier age than fig. 2, *infra*. The cartilage of the terminal phalanx is calcified at its distal extremity, and the calcified portion is enveloped in a thin cap of subperiosteal bone; so that if allowance be made for the diagrammatic nature of the figure, it affords a fair idea of the actual condition of the phalanx as above described. Speaking of this figure, however, the author says, "An der dritten Phalanx ist die Verkalkung schon bis an das distale Ende vorgedrungen, so dass nur das der zweiten Phalanx zugekehrte Ende knorpelig geblieben ist. In der That ist auch schon das ganze Endstück mit einem periostal gebildeten knöchernen Ueberzuge versehen." (Op. cit., p. 279.) It is evident from these words, that the author believes the ossification in this phalanx to have begun at some point other than the distal end of the shaft (presumably near its middle), and to have proceeded thence at different rates towards the two extremities; the only distinction between this and the other phalanges being that here the area of uncalcified cartilage at the distal end has quickly disappeared before the advance of calcification from the centre of the shaft; while at the proximal end the corresponding area still persists; as it does at both ends in ordinary cases. Farther on, he repeats this explanation, and seeing the necessity of providing for the future growth of the bone at its distal extremity, after the disappearance of the supposed cartilaginous head, he rightly assigns the entire task to the periosteum, which has already enveloped the cartilage with its bony cap:—"In der dritten Phalanx schwindet, wie wir früher gesehen haben, der Knorpel an dem distalen Ende vollständig, indem die Verkalkung schon frühzeitig bis an die Oberfläche vorrückt und auch die Kuppel einen knöchernen Ueberzug vom Periost aus bekommt, welches letztere weiterhin auch das sehr geringe Längenwachstum an dem distalen Ende besorgen muss." (*Ibid.*, p. 280.) These extracts will suffice to show that Dr. Kassowitz, while correctly describing the appearances presented by the terminal phalanx in a certain stage of its growth, has failed to draw the proper inferences from them. Had he compared specimens of a somewhat earlier or later stage with the one he has figured, he would probably have been led to recognise the fact that the processes involved in the ossification of this bone spread gradually backwards from the tip; instead of, as he imagines, advancing rapidly towards it.

III. "On a Sun-Spot observed August 31, 1880." By J. N. Lockyer, F.R.S. Received October 26, 1880.

The recent activity in solar spots has enabled me to test the hypothesis I put before the Society on December 12th, 1878, by observing whether the velocity of the up-rush or down-rush of the so-called iron vapour in the sun was registered equally by all the iron lines, as it should be on the received hypothesis.

The observations already made, though few in number, indicate that while motion is shown by the change of refrangibility of some lines, other adjacent lines indicate a state of absolute rest. Thus, in an observation of a sun-spot on August 31st, 1880, when the iron line at 5207·6 was doubly contorted, indicating an ascending and descending velocity of about fifteen miles a second, the two adjacent iron lines at λ 5203·7 and 5201·6, visible in the same field of view, were unaffected.

I send this paper to the Royal Society with all reserve, in order to call the attention of other observers to the point, as I fear it is only too probable that foggy weather will stop all observations here.

IV. "On Methods of Preparing Selenium and other Substances for Photophonic Experiments." By Professor Graham Bell. Communicated by the President.

[Publication deferred.]

November 30, 1880.

ANNIVERSARY MEETING.

THE PRESIDENT in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Council was presented, by which it appears that the total receipts during the past year, including a balance of £1,264 6*s.* 8*d.* carried from the preceding year, amount to £8,592 12*s.* 9*d.*; and that the total expenditure in the same period, including purchase of stock. amounts to £7,397 7*s.* 8*d.*, leaving a balance at the Bankers' on £1,178 9*s.* 7*d.*, and £16 15*s.* 6*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Ansted, David Thomas, M.A.
Bell, Thomas, F.L.S.
Belper, Edward, Lord, M.A.
Brodie, Sir Benjamin Collins, Bart., M.A., D.C.L.
Budd, William, M.D.
Clarke, Jacob Lockhart, M.D.
Cooke, Edward William, R.A.
Erle, Right Hon. Sir William, D.C.L.
Guest, Edwin, D.C.L., LL.D.
Hamilton, The Very Rev. H. Parr, Dean of Salisbury.
Hampton, John Somerset Pakington, Lord, G.C.B.
Lassell, William, LL.D.
Macneill, Sir John, LL.D.
Miller, William Hallowes, M.A., D.C.L.
Napier, James Robert.
Plowden, William Henry Chicheley.
Sharpey, William, M.D., LL.D.
Stephens, Archibald John, Q.C., LL.D.
Taylor, Alfred Swaine, M.D.

On the Foreign List.

Peirce, Benjamin.

Withdrawn.

Robert Bickersteth, Lord Bishop of Ripon.

Change of Name and Title.

Lane-Fox, General, *to* Pitt-Rivers.
Lowe, Right Hon. Robert, *to* Viscount Sherbrooke.

Fellows elected since the last Anniversary.

Attfield, Prof. John, Ph.D., F.C.S.
Beresford-Hope, Alexander James Beresford, LL.D.
Blanford, Henry Francis, F.G.S.
Clifford-Allbutt, Thomas, M.A., M.D., F.L.S.
Dallinger, Rev. William Henry.
Dyer, William Turner Thiselton, M.A., F.L.S.
Godwin-Austen, Lieut.-Col. Henry Haversham.
Graves, The Right Rev. Charles, D.D., Bishop of Limerick.
Hughes, Prof. David Edward.
Jeffery, Henry M., M.A.
Jessel, Right Hon. Sir George, Knt.
M'Coy, Prof. Frederick, F.G.S.
Moulton, J. Fletcher, M.A.
Niven, Prof. Charles, M.A., F.R.A.S.
Northbrook, Thomas George Baring, Earl of, D.C.L., G.C.S.I.
Rae, John, LL.D.
Reynolds, Prof. J. Emerson, M.D.
Tilden, William A., D.Sc.

The President then addressed the Society as follows:—

"Happy is the nation that has no history,"—none, that is to say, in the matter of political events, of diplomatic victories or defeats, of warlike achievements, or other staple topics of record, as history is wont to be written. And such, in fact, has been the state of our own community during the past year. We have no great convulsions to chronicle, nor changes to relate, no controversies with other bodies, nor grievances at issue between us and the State. But, as with a nation, so also with a society, an absence of these more striking, and perhaps superficial, features, is certainly compatible with a healthful internal growth, and with a steady development of the purposes for which our organisation was originally intended.

In the course of the year now ended, we have, naturally, lost some of our elder Fellows; but, numerically at least, our losses have not been so great as during the previous year, nor have they fallen so heavily on our younger members. Among those who have dropped from our ranks, several had already, on account of declining powers, withdrawn from active participation in our proceedings, and had thereby prepared us for their final departure. Among these, two stand prominently forward, men who, through long and faithful service to science and to our Society, deserve to live a long and oft-repeated life in the memory of their friends.

Of Professor Miller it is difficult adequately to speak. His scientific work has been well, but not too fully, described in the obituary notice published in our "Proceedings." Older than myself, and older than other existing officers of the Society, he seemed to belong to an earlier generation. Whether it was due to the number of his years, to the plenitude of his knowledge, to the judicial character of his mind, to his calm but ever ready response to an inquiry, or still more probably to a combination of all these qualities, certain it is

that his friends generally by a tacit consent regarded him as a mentor in the scientific world. And yet, notwithstanding this gravity of demeanour and severity to himself, no one imbibed more thoroughly, nor more liberally contributed to the genial spirit which has always actuated our officers, and even to the good stories which sometimes circulate about a scientific gathering.

Of Dr. Sharpey I might speak in almost the selfsame terms, excepting only that more constant intercourse on the business of the Society, and on other occasions elsewhere, drew our ties of friendship somewhat more closely than in the former case. Dr. Sharpey's life and work are too well told in the obituary notice by an old comrade of his, he was himself too well known, and too widely esteemed, to need any comment of mine; and I will not disappoint my own feelings, nor those of my hearers, by any inadequate words on my part.

The name of Mr. Lassell is one which, whether regarded from the point of view of his scientific work, or from that of private friendship, would be passed over by no one who had the advantage of a knowledge of the former, or of experience of the latter. His name seems to fall in so naturally with those of Herschel and Lord Rosse, that we are apt to class him with the old school. He was, however, of that school only in the best sense; he carried the weight, and earned the dignity which we accord to them; but to his last days, he was as fresh and sympathetic with modern work as the generation which is now succeeding to his. The details of his achievements in instrumental construction, themselves real contributions to science, and of his astronomical discoveries, will be given elsewhere. I will here only add, that in him we have lost a Fellow whose presence was always welcome, and whose assistance and advice were as valuable as they were freely given.

In Professor Ansted we have lost a familiar face, a pleasant writer on science and its accompaniments, and an active promoter of its applications.

I turn now to another who has passed away, and find in him another type of character among our Fellows, namely, Lord Belper. From the member of his family best able to judge, I have the following account of the chief occupations of Lord Belper's life. From his early years his attention was engaged in all questions of political and social interest, especially in free trade, law reform, political economy, and the advance of education. He enjoyed the society of Jeremy Bentham, and an intimate and frequent intercourse with James Mill and John Stuart Mill. Among the friendships formed in his youth, and terminated only by their death, I may mention the names of Macaulay, John Romilly, McCulloch, John and Charles Austen, George Grote, and Charles Buller. And, to use his own words, "Days passed in the society of such men can never be forgotten."

Throughout his life he loved and honoured science. To this he had an hereditary claim. His father, Mr. William Strutt, was for many years the centre of all philosophical and scientific interest in Derby, and the intimate friend and associate of Dr. Darwin. Lord Belper loved to recall this while delighting in the researches of Dr. Darwin's far more illustrious grandson. Although he never devoted himself to any special branch of science, he maintained the deepest interest in all scientific research, and in every new discovery. His life had comprised a period of great and active progress, the development of which he had watched with an interest which appeared to deepen as he grew older; and the great solace of his declining years was the thought (which would often rouse him to enthusiasm) of what had been achieved within his memory towards advancing the comfort and happiness of mankind. During his long connexion with University College, of which he became President at Mr. Grote's death, in 1871, few things gave him greater satisfaction than the generous endowment by his friend, Mr. Jodrell, of the Professorship at that College for the furtherance of Physiological Science.

If in Lord Belper we have an example of one of the many ties which link our body with the outer world, in Mr. E. W. Cooke we have another. Art as well as Politics may have a scientific aspect; and the faithful delineation of the features of nature is an aid with which science could not easily dispense. Mr. Cooke was a keen observer of natural objects, which he viewed with a trained eye and a cultivated mind; and much as he rejoiced in sketching the busy scenes of the seaboard towns of Europe, he never was more happy than when producing, among the rocks, pictures which may almost be described as geological.

On the last of our fallen leaves is inscribed the name of Sir Benjamin Collins Brodie, son of our late President; himself no mean contributor to the science which he cultivated, and no unworthy representative of the firmness of character and independence of thought which have always been connected with his father's name.

Others there are, whom we have lost, to a full tale of twenty; but the narration of their story would be both sad and long.

In regard to our property, I have little to report. The regulations respecting the income arising from the Fees Reduction Fund have been duly carried out by our Treasurer; and the other special funds stand much as at last anniversary.

Several improvements have been effected in the Acton Estate, under the sanction of the Council; and some negociations have been entered into for the sale of the entire estate, which are still pending.

The Society's finances generally are, as the balance-sheet will show, in a healthy condition, and appear to justify the hope that they will suffice for the large claims upon them for printing our publications.

Although we are more concerned with the quality than with the quantity of communications made to the Society, it may not be without interest to observe that the number of papers received this year has been in excess of that in any previous year, at all events since 1872, inclusive. The following is a table of the numbers during the last nine years:—

1872	..	..	99	papers	received.
1873	..	..	92	"	"
1874	..	..	98	"	"
1875	..	..	88	"	"
1876	..	..	113	"	"
1877	..	..	97	"	"
1878	..	..	110	"	"
1879	..	..	118	"	"
1880	..	..	123	"	"

and we may conclude that these have contained good matter from the fact that of the "Philosophical Transactions" for the current year, Parts 1 and 2, already published, contain no less than 900 pages and 33 plates. We have reason to hope that the volume will be completed very early in 1881.

Of the "Proceedings," volume 29 was completed in February, and volume 30 in August last.

In my address last year, I suggested that the hour of our weekly meetings might, perhaps, with advantage, be changed from the evening to the afternoon. That suggestion was approved by the Council, and by their direction a circular was addressed to all the Fellows of the Society, inviting an expression of opinion upon the question. Those of the Fellows who, living at a distance, are unable to attend our meetings, mostly abstained from making a reply. But in the answers actually received, the preponderance of opinion was so strongly in favour of the change, that the Council took the necessary steps for altering the statute by which the hour of meeting was formerly fixed. Notice of the alteration was sent to all the Fellows. The regulation of the hour of meeting is now in the hands of the President and Council. The attendance at our meetings has certainly not diminished since the change, and some of our Fellows, to whom the evening hour was inconvenient, have become constant attendants.

The Council now usually meet at 2.0 P.M., instead of 3.0 P.M., in order to be ready for the meeting of the Society at 4.30 P.M.; and I am happy to add that it has not yet been found necessary to call the Council or the Committee of Papers together on any other than the usual days.

In the permanent staff of the Society no change has taken place.

I regret, however, to record the death of Mr. Henry White, who for many years was chief assistant in the compilation of the great Catalogue of Scientific Papers. At an earlier stage of the work his loss would have been still more serious; but in a long course of training he succeeded so well in imparting his own careful and methodical mode of work to those under him, that the Council felt justified in making trial of his son to take his place. With the result of this trial, as shown in continuing the preparation of a new edition of the catalogue of the Society's Library, the Council have reason to be satisfied.

Of this new edition, the first portion, 220 pages, containing our large collection of "Transactions" and "Proceedings" of Academies and Societies, and other scientific periodicals, is in type, and will shortly be printed off. The verification of titles of our scientific books generally is so far advanced as to warrant the expectation that a large instalment of this portion of the catalogue will soon be in the printers' hands; after which we anticipate no further delay.

In regard to the Library, a question has arisen as to how far purely literary works, which occupy much space, should be retained. Among them there are doubtless some which add neither to the utility nor to the scientific importance of our Library, but there are also some early printed books, bibliographical treasures, which are worthy of a place in any collection. It is proposed to have these carefully put in order, and to place them in a case by themselves. Among these, there may be mentioned :—

Caxton's Chaucer, 1480.
Pynson's Chaucer, 1492.
Speght's Folio Chaucer, 1598.
Ciceronis Officia et paradoxa, *Fust.* 1466, *vellum.*
The generall historie of Virginia, *Lond.* 1632.
Bonifacius. Sextus decretalium liber. *Ven.* 1566–7.

Plautus, 1482.	Seneca, 1490.
Ovid, 1485.	Statius, 1490.
Plutarch, 1485.	Herodotus, 1494.
Homer, 1488.	

For bringing into prominence these as well as other features of our miscellaneous, *i.e.*, non-scientific, books, we are greatly indebted to the care and knowledge brought to bear on the subject by Mr. Tomlinson, and by our Treasurer.

Although it is doubtless undesirable to propose, without sufficient cause, alterations in our Statutes, or even in our practice, it is still often worth while from time to time to discuss questions involving such alterations in order that we may be prepared for a deliberate judgment whenever occasion may arise. Among such questions there

is one upon which I have often heard opinion expressed, and upon which opinion has always weighed in the same direction: I allude to the period of office of those elected to serve on the Council of the Society. By the terms of our charter ten of the ordinary members retire every year; and as it is our custom to remove six according to seniority and four in respect of least attendance, it rarely happens, although the contrary is possible, that any Fellow, except those holding the posts of President, Treasurer, or Secretary, should remain in office more than two years. Experience, however, appears to show, that for a member serving on the Council for the first time, there is so much to learn, so many heads of business demanding attention which do not in general come before the Fellows at large, that his first year is occupied quite as much in ascertaining his duties as in actively performing them. This objection is in some degree met by selecting for the ten incoming members five who have served before and five who have not so served; but, nevertheless, there is usually an interval of several years between two periods of office, and as a matter of fact we often lose a member of Council at the moment when his advice is becoming most valuable to our body.

I am aware of the great convenience attaching to our present impersonal mode of selecting the members to retire in each year, and am not at present prepared to suggest any specific alteration. But the great confidence which the Society has, especially of late years, placed in its more permanent officers, and the power which naturally accrues to them from the comparatively short tenure of office by the other Members of Council, appear to me to be points of which the Society should not lose sight. On the part of the officers, I think it right to state that we are very sensible both of the honour which is thus done to us, and of the responsibility which is thereby entailed, and that we hope never to discredit the one, nor to abuse the other. And having said so much, we are quite willing to leave the matter in the hands of the Society to be taken up whenever they see reason so to do.

It will be in the recollection of the Fellows that the position of the Royal Society in respect of the Government Fund of £4,000 per annum is different from that in relation to the Government Grant of £1,000 per annum. In the latter case the sum is placed unreservedly in the hands of the Society for promoting scientific investigation, subject only to an annual report to the Treasury of the sums granted; and, in administering it, the Society has in no case applied it to the personal remuneration of the applicant. In the former case, the Society has been requested to advise the Science and Art Department as to the distribution of the grant, not only for the direct expenses of investigations but also for personal remuneration for the time expended on them, whenever the circumstances and wishes of the applicant appeared to render this desirable. The responsibility of this advice lies with a

Committee similar to that of the Government Grant, but with the addition of the Presidents of certain learned bodies and societies, nominated for that purpose by the Government.

The recommendations made by the Committee each year are annually published in the "Proceedings," so that the public will have had full information as to the distribution of the grant; while the Fellows have the opportunity of seeing the nature of applications made, and the extent to which it has been found practicable to meet them, as recorded in the minutes of the Council of the Society.

One of the points which is perhaps beset with the greatest difficulty is that of the so-called "personal" grants. On the one hand, it has been argued that it is desirable to enable the man of small means to devote to research a part of his time which he could not otherwise afford to give; but, on the other, the question has been raised whether it be wise, even in the interests of science, to encourage anyone not yet of independent income to interrupt the main business of his life. It is too often assumed that a profession or a business may be worked at half speed, or may be laid down and taken up again, whenever we like. But this is not so, and a profession temporarily or even partially laid aside, may prove irrecoverable; and the temptation to diverge from the dull and laborious path of business may prove to have been a snare. Without proposing to exclude from possible aid in some shape or other those cases where personal assistance may be safely offered, it has been suggested that many such cases may be practically met by grants for the employment of an assistant, instead of grants to the applicant himself.

There is another fundamental difference between the position of the Government Grant of £1,000 per annum and the Government Fund of £4,000 per annum, which appears to me to be of material importance in the interests of science. The former is an absolute grant from the Treasury made to the Society for scientific purposes. It may be used wholly, or in part, during the year in which it is made, and the balance, if any, may be carried over by the Society to the next or even to succeeding years. The latter is a vote to the Science and Art Department, on the disposal of which the Society is consulted. Like all other similar votes, any unused balance reverts to the Treasury, and is to that extent lost to the purpose for which it was intended. I cannot help thinking that, if any such balances could be reserved and kept in hand, provision might be made for some larger purposes than those to which the fund has hitherto been devoted. And, even if having this end in view, the Committee should not see its way to recommend some of the smaller applications, it may be fairly questioned whether the smaller grants might not find a more appropriate place among those of the Donation Fund of this Society, or of the British Association, or among some of those separate funds which, through the

liberality of individuals, are now growing up among the special societies.

I am glad to record the fact that, upon the recommendation of men of science, Her Majesty has been pleased to grant pensions on the Civil List to the widows of two of our late Fellows, viz., to Mrs. John Allan Broun and to Mrs. Clifford.

Last year two volumes containing a collection of the late Professor Clifford's general lectures and essays were brought out. It is hoped that during the present winter a collection of his mathematical papers will be published. The contributions to science by the late Professor Rankine have recently been placed in the hands of the public. While very sensible of the obligations under which the scientific world is placed by these posthumous publications, I cannot refrain from alluding to our obligations, even greater if possible, to those who during their lifetime are willing to re-issue their own scientific memoirs, and to give us thereby not only the convenience of ready access, but also the advantage of their own subsequent reflections on the subjects of which they have treated. And at this moment I desire to mention more particularly the mathematical and physical papers of our Senior Secretary, Professor G. G. Stokes; and, while expressing our gratitude for the volume which has already appeared, I would express also our sincere hope that another instalment from the same source may shortly follow.

Among the subjects which at one period of the last session of Parliament engaged the attention of the Government was that of the law relating to vaccination; and a Bill was introduced intended to remove some of the practical difficulties in carrying out the existing law. While fully admitting the difficulties in question, the remedy proposed appeared to trench so closely upon the application at least of a scientific principle, and at the same time to be so important in its practical aspect, that I ventured (although the Council was not sitting) to consult the Presidents of the Colleges of Physicians and of Surgeons, and that of the Medical Council, about addressing the Government on the subject. This resulted in a joint deputation to the President of the Local Government Board, in which I took part as President of the Royal Society. The Council on my reporting the matter to them at their first meeting after the recess, expressed their approval. The Bill in question was withdrawn.

The Royal Commission on Accidents in Coal Mines, the appointment of which I mentioned in my address of last year, have been occupied principally in bringing together a body of valuable evidence on the causes and prevention of accidents in mines generally. The Commission have also visited a number of mines in which serious accidents by explosion have taken place, or in which certain phenomena connected with the occurrence of fire-damp were to be studied. They

have also instituted a series of experiments on the behaviour of various safety lamps in mixtures of natural fire-damp and air. These experiments they are about to renew during the winter. They also contemplate carrying out experiments in blasting rock and coal by methods which will check the production of flame, and which are thereby calculated to obviate the danger of igniting fire-damp.

The report of the voyage of H.M.S. "Challenger," to which the scientific world has been looking forward with so much interest, is now so far advanced that one volume of the "Zoological Memoirs" will appear immediately. In addition to this a second volume may be expected within a year. The first volume of the whole work, "containing a short narrative of the voyage, with all necessary hydrographical details, an account of the appliances and methods of observation, a running outline of the results of the different observations; and a chapter epitomising the general results of the voyage," together with the second volume containing the meteorological, magnetic, and hydrographic observations, will probably be published within the same period. "The general report on the zoology of the expedition will consist of about fifty distinct memoirs, which will occupy from ten to twelve volumes." It has been arranged "to print the Zoological Reports as they are prepared, and to publish them as soon as a sufficient bulk of memoirs is ready to form a volume. Copies of each memoir may also be had separately, in order that working naturalists may have them in their hands at the earliest possible date." Two more volumes on the geology and petrology, and one on the general chemical and physical results, will probably complete the series. Into zoological details I am not competent to enter, but one among them is of great interest, namely, the fact that notwithstanding the pressure and absence of light, there is no depth-limit to animal life.

As the Council of the Meteorological Office is nominated by the Council of the Royal Society, and as the Annual Report of the Office is submitted to the Royal Society, I think it right to mention a few points connected with the work of that Department during the past year.

1. A method of recording the duration of bright sunshine by the charring of an object placed in the focus of a glass sphere, freely exposed to the rays of the sun, was devised by Mr. J. F. Campbell, of Islay, in 1856; and instruments, being modified forms of that originally proposed, have been employed for some time at Greenwich, at Kew, and at a few private observatories. Certain difficulties in adjusting the paper about to be charred to the path of the burning spot, which had hitherto prevented the adoption of Mr. Campbell's invention as a part of the ordinary equipment of a meteorological observing station, have been at last successfully overcome by an arrangement designed by Professor Stokes; and thirty stations in the

British Isles have now been supplied with instruments of the pattern proposed by him. We may thus hope to obtain in future a sufficient record of a meteorological element, which is of primary importance in its relations to agriculture, and to the public health, but which has hitherto been very imperfectly registered.

2. The climatology of the Arctic regions, in addition to its importance as a part of the general physics of the globe, possesses a special interest in connexion with geographical exploration. As a contribution to our knowledge of this subject, the Meteorological Office has entrusted to Mr. R. Strachan the task of bringing together, and discussing on an uniform plan, the results of the observations taken at intervals during the last sixty years, in the region extending from the meridian of 45° W. to that of 120° W., and from the parallel of 60° to that of 80°, either at land stations or at the winter quarters of British and American expeditions. A considerable portion of this discussion has been already published; the remainder may be expected in the course of next year.

3. Another publication of the Meteorological Office may be mentioned as serving to mark the advance in meteorological theory, which has been achieved during the last fifteen years. The old "Barometer Manual and Weather Guide" of the Board of Trade has been replaced, so far as it relates to the weather of the British Isles, by a work entitled "Aids to the Study and Forecast of Weather," prepared under the direction of the Meteorological Office by the Rev. W. Clement Ley. Though some of the views put forward in the later work may, perhaps, be regarded as not sufficiently established by observation, yet a comparison of the two works cannot fail to leave upon the reader's mind the impression that in the interval between their respective dates of publication, some real progress has been made in meteorology. Perhaps this is most conspicuous in the enlarged ideas that are now entertained concerning the conditions upon which the changes of weather depend. Local weather was first discovered to be contingent upon travelling areas of disturbance, each of which averaged many hundreds of miles in diameter, while, at the present time, the relation of these areas to one another, as parts of a single terrestrial system, has become a prominent topic of inquiry. If meteorology has thus been, to a certain extent, rescued from the ever accumulating chaos of numerical tabulations, which threatened to engulf the whole science, the improvement is mainly due to the development in recent times of the synoptic study of weather over large regions of the earth's surface, to which so great an impetus has been given by the extended facilities of telegraphic communication.

4. Balloon ascents, with a view to military purposes, are now systematically carried on under the direction of the War Office; and the endeavour has been made to take advantage of these ascents for

observations of the thickness of the aërial current which causes our winds, and of the peculiarities of the currents above it in the upper strata of the atmosphere. The military authorities have offered their co-operation in the most cordial manner; but the attention of an aëronaut is often so much engrossed by the operations necessary for working his balloon, that he has but little leisure for taking systematic records. Nevertheless, observations of considerable interest have already been obtained, relating especially to the velocity and direction of the upper air currents; and there can be no doubt that a continuance of such observations affords the best prospect at present open to us of adding to the very scanty knowledge which we possess of the movements of the atmosphere, even at a moderate height above the earth's surface.

Among the various duties which the President of the Royal Society is called upon to fulfil there are those of a Trustee of the British Museum; and, as an operation of great importance to Science, namely the removal of the natural history collections to the new building at South Kensington, is now going on, the Fellows may be interested to hear what progress has been made in the work.

The plans for the new building were approved as long ago as April, 1868; but the works were not commenced until the early part of 1873. Their progress was retarded by difficulties in the supply of the terra cotta with which the building is faced within and without, and in which the mouldings of arches and other ornamental features are executed.

The building was finally handed over to the Trustees in the month of June of the present year. It contains cases for three only of the Departments for which it is intended, namely, Mineralogy, Geology, and Botany; the necessary funds for the Zoological Department not having been yet voted. As the latter collections are equal in bulk to the other three collectively, it follows that half only of the new building can at present be actually occupied. The removal of the collections for which cases had been provided, commenced in the last week of July, and was virtually completed by the end of September.

Geology, which was very inadequately displayed in the old building, is now more commodiously accommodated than heretofore. It occupies a gallery 280 feet in length by 52 in breadth, forming the ground floor of the east wing of the new museum, together with eight other galleries covering an area of 200 × 170 feet at the back, and admirably adapted for the exhibition of the specimens. One of these galleries will be devoted to the illustration of stratification.

The principal part of the minerals has been moved and replaced in the cases in which they were arranged in the old building. This collection now occupies the first floor of the east wing of the new museum, and the space devoted to it is 280 × 50 feet in area. It is already arranged for exhibition.

The Botanical collections are placed in the gallery over the minerals, where the space for exhibition and the conveniences for study are much greater than in their old quarters.

The construction of the cases for the Zoological specimens, and the ultimate removal of these collections, must depend upon the amount of the Parliamentary vote for the purpose; but under the most favourable conditions it can hardly be hoped that this Department can be open to the public or to students in less than two years from the present time.

The "Index Museum," designed by Professor Owen, will form a prominent feature in the new museum. The object of it, in his words, is "to show the type characters of the principal groups of organised beings;" and "to convey to the great majority of visitors, who are not naturalists, as much information and general notions of its aim as the hall they will first enter and survey could be made to afford."

One of the principal difficulties attending the transfer of the Natural History Departments to a separate building consists in the provision of books for the use of the keepers and their staff, as well as for students who may visit the museum. Hitherto the separate collections of books, known as departmental libraries, supplemented as occasion might require from the main library of the museum, have sufficed for all purposes. But now, when the departmental libraries have to stand by themselves, it is impracticable to carry on even the current work of arrangement without additional resources. For an adequate supply of the necessary works a very large outlay would be required, supposing that the works were in the market. But many of them are out of print and have become scarce; and a large grant of public money would perhaps raise the market price almost in proportion to its magnitude. This being so, it has been thought best, on the whole, by the Government to make an annual grant to be expended from time to time as favourable opportunities for purchase may offer. If it should prove possible, and on other grounds desirable, to allow the Banks' Library to follow the collections with which it has always been practically connected, the wants of the Natural History Departments would (so far as books up to the date of its bequeathment are concerned) be in a great measure supplied.

Another of the duties which falls officially on your President is to take part in the organisation of technical education as promoted by the City and Guilds of London Institute, which is now incorporated under the Companies Acts, 1862–80 as a registered association, and of which the Presidents of the Royal Society, the Chemical Society, the Institute of Civil Engineers, and the Chairman of the Council of the Society of Arts, are members. In the Memorandum and Articles of Association of the Institute, its objects are fully set forth. They may be summarised under the following heads:—

1st. The establishment of a central technical institution for instruction in the application of science and art to productive industry.

2nd. The establishment of trade and technical schools in London and in the country.

3rd. The development of technical education by means of examinations held at the Central Institution, or at other places.

4th. To assist by means of grants existing institutions in which technical education is being promoted.

5th. To accept gifts, bequests, and endowments, for the purposes of the Institute.

The Institute is supported by subscriptions from sixteen of the City Companies, of which the largest contributors are the Mercers, Drapers, Fishmongers, Goldsmiths, and Clothworkers.

The Institute has been in active operation not much more than a year, and during the last six months the work of the Institute has developed considerably in each of its several departments. These may be considered under the following heads:—

1. Technical Instruction.
2. Examinations in Technology.
3. Assistance to other Institutions.

1. Since November last, courses of lectures and laboratory instruction have been given in the temporary class rooms of the Institute, at the Cowper Street Schools, under the direction of Professor Armstrong, F.R.S., and of Professor Ayrton. The subjects of instruction have included Inorganic and Organic Chemistry, with special reference to their industrial applications; Fuel, Electro-depositions of Metals, and Photographic Chemistry; General Physics, Steam, Electrical Engineering, Electrical Instrument Making, Electric Lighting, Weighing Appliances, and Motor Machinery.

During the term ending July last, the number of tickets issued to students, most of whom belonged to the artizan class, exceeded three hundred. A considerable accession of students is expected as soon as the building in Tabernacle Row, the plans of which are already settled, shall be erected. This building, which is estimated to cost £20,000, will provide accommodation for schools of Technical Physics, Technical Chemistry, and Applied Mechanics. Many of the day students at these classes are pupils of the Cowper Street Schools, and it is hoped that, by adapting the course of technical instruction to be given in the College to the wants of these boys, a very complete technical school for the children of artizans will have been established.

The evening lectures and laboratory instruction, which are more advanced and more special, are attended very largely by external students, for whom the present temporary accommodation is already too limited.

At Kennington, schools have been established in which practical

instruction is given in various art subjects, such as Painting and Drawing, Modelling, Designing, and Wood Engraving. These schools are attended by both sexes, and are under the immediate direction of Mr. Sparkes. The numbers in attendance last term were as follows:—

Wood Engraving..............	8 Students,	3 Men,	5 Women.
Modelling.....................	28 ,,	26 ,,	2 ,,
Drawing and Painting from Life.	42 ,,	19 ,,	23 ,,
Designing.....................	33 ,,	3 ,,	30 ,,

The Central Institution for instruction in the application of the higher branches of science to industrial pursuits is about to be erected on a plot of ground in Exhibition Road, granted by the Commissioners of 1851. The construction of this building, which, when completed, will cost £50,000, has been entrusted to Mr. Alfred Waterhouse, who is now engaged in the preparation of plans.

2. In the year 1879, the examinations in Technology, which had been initiated by the Society of Arts, were transferred to this Institute. Various changes were introduced into the regulations. New subjects were added, and, in order to stimulate the teaching of Technology throughout the country, the principle of payment to teachers on the results of the examinations was adopted. The encouragement thus afforded to teachers gave a great impetus to the formation of classes throughout the country in technological subjects. Last year the number of candidates for examination was 202, while at the recent examination, held in May, 816 candidates presented themselves, of whom 515 satisfied the Examiners. During the last few months the number of classes throughout the country, in which technical instruction is being given, has considerably increased, and, judging from the returns already received, there is reason to believe that the number of candidates who will present themselves for examination next May will be much greater than in either of the preceding years. The new programme, which is just issued, contains a syllabus of each subject of examination, and every effort has been made, short of testing the candidate's practical skill, to make the examinations as efficient as possible. To obtain the Institute's full certificate, each candidate is required to give evidence of having obtained some preliminary scientific knowledge.

3. In order to take advantage of efforts that are already being made to advance technical education, the Institute has given sums of money for specific objects to several institutions in which technical instruction is provided. The schools, colleges, and other bodies to which grants have been made by this Institute, are University College and King's College, London, the School of Art, Wood Carving, and Mining Association of Devon and Cornwall, the Nottingham Trade and

Science Schools, the Artizans' Institute, the Birkbeck Institute, the Lancashire and Cheshire Union, and the Horological Institute.

The Artizans' Institute gives practical instruction in several of the humbler crafts in which artizans are engaged, such as carpentry, zinc work, and plumbers' work; and corresponds, therefore, to some slight extent, with the apprenticeship schools of the Continent, from which, however, it differs in many important particulars. A similar experiment is being tried at the Horological Institute, where, at the expense of the Guilds, classes have been organised in which apprentices are practically instructed in the various branches of the watch-making trade.

It is found that the demand for technical instruction in London and throughout the provinces is very great, and the efforts that have been so far made by the City and Guilds of London Institute, have afforded considerable satisfaction to artizans and others engaged in industrial pursuits, and promise, when further extended, to be of the utmost service in the development of technical education in this country.

Turning now more particularly to the progress and the applications of science, I venture to make mention of a few topics which have come under my own observation:—

The aspect of spectrum analysis has become much complicated by two sets of facts. First, the increased dispersion, the improved definition, the enlarged electrical power at our command, and, above all, the substitution of photography for eye observations, have revealed to us an almost overwhelming array of lines belonging to each substance. And, secondly, the same means have shown that many substances present different spectra when in different molecular states. These complications have led spectroscopists to seek some relief in theories of simplification. Lecoq de Boisbaudran, Stoney, Soret, and others, have suggested that many of the lines, or groups of lines, may be regarded as the harmonics of a fundamental vibration; and they have shown that in certain cases this view will account for the phenomena observed. Professors Liveing and Dewar have contributed largely to our knowledge of the subject, by their observations on the reversed lines. Looking in another direction, Mr. Lockyer considers that in increased temperature we have the means not only of resolving compound bodies into their elements, but even of dissociating bodies hitherto regarded as elementary into still more simple substances. There still remain serious difficulties connected with Mr. Lockyer's views; but it is to be hoped that his indefatigable energy will in some way or other ultimately overcome them.

The outlying parts of the spectrum, beyond the visible range, must always be a subject of interest; and while MM. Cornu and Mascart, and others, have extended our knowledge of the ultra-violet end,

Captain Abney has opened out to us a new region beyond the red. Lord Rayleigh and others before him have, however, proved that there must be a limit at the least refrangible end of the spectrum. Professor Stokes, long since, noticed the difference in length between the spectrum of the sun and that of the electric arc; and M. Cornu has recently shown by observations at elevated stations that the great rapidity of atmospheric absorption must preclude the hope of any great extension of the solar spectrum toward the more refrangible end.

The striking advances made in electricity during the last few years, and marked by, among other things, the inventions of the telephone and the microphone, have been followed by a step not less daring in its conception, nor less successful in its execution; I allude, of course, to the photophone, the result of the researches of Mr. Graham Bell and Mr. Sumner Tainter. The principle of this instrument is already known. A powerful beam of light is first thrown upon a flexible mirror, the curvature of which is modified through vibrations set up in it by the human voice. The reflected beam is then received by a selenium "cell," forming part of an electric circuit. The intensity of the light so received, and with it the resistance in the circuit due to the selenium, varies with the varying curvature of the flexible mirror. A large parabolic mirror is used at the distant station to concentrate the light on the selenium "cell;" and a telephone in the circuit reproduces the variations in the form of sound.

Mr. Bell has, however, also shown that rays from the sun, or an electric lamp, when rendered intermittent by any convenient means, will set up in a plate of almost any substance vibrations corresponding to the intermittence. The substances as yet tried are: metals of various kinds, wood, india-rubber, ebonite, and many others, and among them zinc appears to be one of the best suited for the purpose. This result, which is independent of any electric action, is, perhaps, due to heat rather than to light.

In these, as in many other issues of scientific research, we can hardly fail to be impressed by the almost inexhaustible resources which lie ready to hand, if we only knew how to use them, for the interpretation of nature, or for the practical purposes of mankind.

During the past year Professor Hughes employed his induction balance for the detection of very minute impurities in small masses of gold. Mr. Preece also has shown how slight increments of temperature in fine wires transmitting telephonic currents of electricity, will suffice to reproduce sonorous vibrations; and even articulate speech at a distant station by their influence on thin platinum wires, only six inches in length.

Mr. Stroh has shown that, at the point of contact of two metals carrying strong electric currents, adhesion takes place, varying with

the nature of the surfaces in contact; and that many of the effects at points of contact, previously attributed to induction, may be due to the peculiar action now for the first time brought under notice.

It is worthy of record, that two Atlantic cables have been successfully laid during the present year; but success in cable-laying has become so much a matter of course, that its occurrence has attracted little public attention. Two cables, each of more than 500 miles in length, have been laid across the Mediterranean; and the Cape Colony has been placed in telegraphic communication with this country, by a cable of not less than 4,400 miles.

Constant attention is paid in the General Post Office to the introduction of improved methods for the furtherance of the telegraphic communication throughout the country.

Steady progress has been made in bringing the electric light into practical use. The illumination of the Albert Dock of the London and St. Katherine's Dock Company, the Liverpool Street Station of the Great Eastern Railway, the St. Enoch's Station of the Glasgow and South-Western Railway, and last, but not least, that of the reading room of the British Museum, has become an accomplished fact; while the city authorities have decided to extend the use of this light over various thoroughfares under their control. The subdivision of the light for domestic purposes is a problem which appears to have found a solution in the incandescent carbon lamp of Mr. Swan. Beside this, Mr. J. H. Gordon has devised, for the same purpose, a very ingenious application of rapid sparks from alternating machines, such as that of De Méritens, to produce incandescence in refractory metals. Lamps constructed on this principle completely fulfil the conditions of subdivision, but some difficulties of detail still retard their adoption for general use. There is, however, every reason to hope that the experience already gained, and the intelligence at present brought to bear upon the subject, will before long supply us with more than one form of domestic light.

The chief question of interest which has occupied the attention of the Iron and Steel Institute has been the adaptation of the "basic" process to the production of steel from pig metal containing a considerable percentage of phosphorus. Hitherto only pure hæmatite and spathic ironstones have been used for the production of steel; but it has now been shown that, by the employment of basic linings and basic slags, the metal is almost completely cleared of its phosphorus, and that steel of good quality may be produced from inferior ore.

The Conference on Lightning Conductors, composed of delegates from the Royal Institute of British Architects, the Society of Telegraph Engineers, the Physical Society, and the Meteorological Society, is steadily pursuing its labours. A large mass of facts has been accumulated; several leading questions have been decided; and

it is hoped that, in the course of the coming year, the Report of the Conference will be issued.

One of the most interesting, and at the same time useful, applications of the dynamo-machines, is that of transmitting mechanical power to spots, or under circumstances, where the ordinary appliances cannot be conveniently used. Perhaps one of the most remarkable instances of the application of the principle, is that by Dr. Werner Siemens to the propulsion of railway carriages in Berlin. Other applications will doubtless by degrees extend themselves over a wide range of industry; especially in localities where water-power is abundant.

Our Fellow, Dr. C. W. Siemens in London, and M. De Méritens in Paris, have demonstrated the use of the high temperature of the electric arc in fusing refractory metals. The method of operation, while peculiarly convenient for laboratory purposes, and for demonstration, promises to be capable of extension, even to the large demands of commerce and manufacture.

I should not, moreover, omit mention of the very beautiful experiments by Dr. C. W. Siemens, on the effect of the electric light on the growth of plants, on the opening of flowers, and on the ripening of fruit. On this subject we hope to hear more after the experiments which, already commenced, he contemplates continuing during the coming winter.

I am not sure how far the fact is known to the Fellows of the Royal Society, that the Society of Telegraph Engineers have thrown open to the scientific world a remarkable collection of books on electrical science, collected by our late Fellow, Sir Francis Ronalds, and bequeathed by him to that Society. The catalogue, compiled by the collector, is a monument of concentrated and well-directed labour.

As regards the Transit of Venus in 1874, the printing of the observations is complete for the two groups of stations in the Sandwich Islands and Egypt, and that for others is in progress.

Preparations are already being made with a view to the observation of the Transit of Venus in 1882. As a preliminary step for this operation, as well as for general purposes, it had been decided that the longitude of the Cape Observatory should be definitively determined by telegraphic connexion with Aden, which place is already telegraphically referred to Greenwich; and, notwithstanding a temporary interruption on the land line, Cape Town–Durban, it may be hoped that the determination will be effected at no distant period. Mr. Gill is prepared to undertake the main share of the work. With the same objects in view, on the urgent representation of the Astronomer Royal, it has also been determined to connect one of the Australian Observatories with Greenwich, through Madras, the longitude of which is well known; and this operation will be very much facilitated by the share which Mr. Todd, Government Astronomer

and Superintendent of Telegraphs at Adelaide, would be prepared to take in it under the auspices of his Government. The eastern boundary of the colony having been defined by Imperial Act as the 141st meridian, a wish has been expressed officially for the accurate connexion of Adelaide with Greenwich, independently of the Transit of Venus.

The Astronomer Royal has explained in detail the preparations which he considers necessary, so far at least as this country is concerned, for the effective observation of the transit, and he has introduced several alterations in the plan which he had formerly suggested. The experience of the transit of 1874 points to the desirability of sacrificing something in the magnitude of the parallax-factor for the sake of securing a higher elevation of the sun; thus, for retarded ingress, Sir George Airy had at first proposed to refer principally to the coasts of the Canadian Dominion and the United States of North America, where the sun's elevation is from 15° to 18°; he now proposes to substitute for this the whole chain of West India Islands, from the eastern extremity of Cuba to Barbadoes, or stations on the neighbouring continent of Central America. Bermuda is also included as a favourable point for observation. Most, if not all, of the longitudes required have been determined with great precision by the Hydrographic Department of the United States. For ingress accelerated, Sir George Airy relies entirely upon stations in the Cape Colony. For the accelerated egress, all the stations suggested for ingress retarded will be available. For egress retarded, although the fixed Observatories at Melbourne and Sydney will contribute to the observation of the phenomenon, they will have the sun at a somewhat low elevation (10—14°); it is therefore proposed to rely mainly upon New Zealand, with which we are in telegraphic communication *viâ* Sydney. Considerable correspondence has taken place on the subject of Australian longitude, and it is expected that the necessary steps to effect the connexion of one of the Observatories, probably Adelaide, with Madras, will be taken early in the ensuing year.

Sir G. B. Airy has completed the laborious calculations in his Numerical Lunar Theory, from which the corrections to the coefficients of Delaunay's Lunar Theory are to be deduced; and in connexion with this work, he has made an investigation of the value of the Moon's Secular Acceleration, for which he finally obtained the value 5″·477, thus confirming the results obtained by Professor Adams, and subsequently by Mr. Delaunay. On this important question, Professor Adams has also published an investigation. ("Monthly Notices," vol. xl, Nos. 411 and 472.)

A new determination of the Physical Libration of the Moon from a large number of lunar photographs taken with the De La Rue reflector at the Oxford University Observatory has been recently made by

Professor Pritchard, the result being to indicate the existence of a small rotational inequality.

Messrs. J. Campbell and Neison have made use of the Greenwich Observations, 1862 to 1876, to determine the Lunar Parallactic Inequality, from which they deduce for the value of the Solar Parallax, 8″·778, or 8″·848, according as the existence of a forty-five year inequality, apparently indicated by the observations, is admitted or not ("Monthly Notices," vol. xl, Nos. 7 and 8). The Sun's Parallax has also been determined by Mr. Downing, from N.P.D. observations of Mars at Leyden and Melbourne, in 1877. The value thus found is 8″·96. ("Astronomische Nachrichten," No. 2,288.)

In continuation of his researches on tidal retardation from the action of a satellite on a viscous planet, Mr. G. H. Darwin has investigated the secular changes in the orbit of a satellite, deducing the early history of the earth and moon from the time when they were initially in contact, each revolving in the same period of from two to four hours. This leads to the suggestion that the moon was produced by the rupture of the primeval planet. In another memoir, Mr. G. H. Darwin gives analytical expressions for the history of a planet and a single satellite. ("Phil. Trans.," 1879, "Proc. Roy. Soc.," vol. xxx, pp. 1, 255.)

An important work in connexion with the United States Northern Boundary Commission has been published by Mr. Lewis Boss, on the Declination of Fixed Stars. The systematic corrections to some seventy catalogues have been discussed, and, from the mean of the whole, standard declinations of 500 stars have been deduced.

Dr. Gould's "Uranometria Argentina" and M. Houzeau's "Uranométrie Générale," are of especial value as giving important information on the brightness and distribution of the stars in the southern hemisphere.

Interesting results as to the diameters of satellites have been obtained by Professor Pickering from photometric observations, on the assumption that their albedos do not differ greatly from those of their respective primaries. ("Annals Harvard College Observatory," vol. xi.) He has further investigated, on somewhat similar principles, the dimensions of the fixed stars, with especial reference to binaries and variables of the Algol type. ("Proc. Amer. Acad.," vol. xvi.) Professor Pickering has also commenced a photometric survey of the heavens in which the brightness of every star visible to the naked eye is to be determined. He has further undertaken a search for planetary nebulæ by a new method, in which, by the use of a direct-vision prism in front of the eye-piece, the nebula is at once detected by its monochromatic spectrum, focussing a point of light instead of a coloured line as in the case of a star. About a hundred thousand stars have been examined, and four new planetary nebulæ

have been detected. ("American Journal of Science," October, 1880.)

From the grouping of the aphelia of certain periodic comets, Professor G. Forbes has inferred the existence of two ultra-Neptunian planets, and has indicated their approximate positions. ("Trans. Roy. Soc., Edinburgh.") Mr. D. P. Todd has deduced from the perturbation of Uranus, a position for an ultra-Neptunian planet closely agreeing with that found by Professor G. Forbes. So far, the search for the hypothetical planet with the 26-inch Washington refractor has been unsuccessful. ("American Journal of Science," September, 1880.)

Professor Bredichin's researches on the tails of comets have led him to the classification of these appendages according to the value of the solar repulsive force which would have generated them. Having discussed the forms of the tails of thirty-three comets, he finds that they belong to three types, corresponding respectively to repulsive forces 11, 1·4 and 0·3 (the sun's gravitation being taken as 1), and adopting Zöllner's hypothesis of a repulsive force, due to electricity and inversely proportional to the specific gravity, he infers that the tails of the three types are composed respectively of hydrogen, carbon, and iron. In the case of the second and third types other elements of nearly the same atomic weight may replace or be mixed with the carbon and iron, and in such a comet as Donati's a number of substances may be mixed in the tail, which will consequently spread out in the plane of the orbit. The first type composed of hydrogen will always remain separated from the others. ("Annales de l'Observatoire de Moscou," vols. iii—vi.)

The appearance, at the beginning of this year, of a great comet in the southern hemisphere, recalling by the length of its tail and the smallness of its head the remarkable comet of 1843, has excited great interest, more especially as it was found that the orbits of the two comets were sensibly the same. The observations of the comet of 1843, however, do not appear to be compatible with so short a period as thirty-seven years, and Professor Oppolzer has shown that the action of a resisting medium would not meet the case. ("Astronomische Nachrichten," Nos. 2314, 2315.) Under these circumstances Professor D. Kirkwood has suggested that the two bodies may be fragments of one original comet, viz., that of 370 B.C., which is said to have separated into two parts like Biela's comet. ("Observatory," No. 43.) Five other comets (including Faye's periodical comet) have been discovered this year, but two of them were lost through cloudy weather before a second observation could be made.

In astronomical physics Mr. Huggins has obtained photographs of stellar spectra, which establish the existence of a remarkable group of nine bands in the ultra-violet, probably due to hydrogen, and further

lead him to an arrangement of the stars in a continuous series according to the breadth and marginal differences of the typical lines, particularly of the K line. Mr. Lockyer continues his researches on dissociation, as indicated in solar outbursts, and in connexion with this work is engaged on a systematic observation of the spectra of sun-spots. At the request of the Committee on Solar Physics, corresponding observations are being made at Greenwich.

From the series of Greenwich photographs of the sun, 1874—1879, the mean heliographic latitude of spots and mean distance from the sun's equator, have been deduced for each rotation and for each year. ("Greenwich Spectroscopic and Photographic Results, 1879.)"

A fine 36-inch silver-on-glass reflector has been recently constructed by Mr. Common, and with this instrument he has obtained photographs of Jupiter, showing the red spot, and of the satellites. ("Observatory," No. 34.)

At the outset of an undertaking one figures to oneself in imagination what may be done; towards the close of it one sees in actual fact what has been done. In commencing this address I had hoped to say something of the progress of mathematics; before bringing it to a conclusion, I find my space filled and my time exhausted. How far the good intentions of this year may be realised in the next, cannot yet be seen; but the difficulties of a task do not always diminish the fascination of making an attempt.

On the motion of Mr. Scott Russell, seconded by Mr. Merrifield, it was resolved:—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals:—

The Copley Medal has been awarded to Professor James Joseph Sylvester, F.R.S. His extensive and profound researches in pure mathematics, especially his contributions to the Theory of Invariants and Covariants, to the Theory of Numbers, and to Modern Geometry, may be regarded as fully establishing Mr. Sylvester's claim to the award of the Copley Medal.

A Royal Medal has been awarded to Professor Joseph Lister, F.R.S. Mr. Lister's claims to the honour of a Royal Medal are based upon his numerous and valuable contributions to physiological and biological science during the last thirty years.

By permission of its author, the Fellow of the Society best qualified, by his own extensive researches on the germ theory, to form a judgment, I quote the following account of Professor Lister's work and achievements:—

"In 1836 and 1837 it was proved independently by Cagniard de la Tour and Schwann, that vinous fermentation was due to the growth and multiplication of a microscopic plant. At the same time Schwann described experiments which illustrated and explained the conditions, now well known, by which flesh may be preserved from putrefaction. But Schwann's researches were overshadowed by the views of accepted authorities, and they continued so up to the publication of Pasteur's investigations. From this point forward the view gained ground that putrefaction is the work of floating microscopic organisms; and that if air be thoroughly cleansed of its suspended particles, neither its oxygen, nor any other gaseous constituent, is competent to provoke either fermentation or putrefaction.

"Condensed into a single sentence, the merit of Mr. Lister consists in the generalisation, to living matter, of the results obtained by Schwann and Pasteur with dead matter. He began with cases of compound fracture and with abscesses. In simple fracture the wound is internal, the uninjured skin forming a protecting envelope. Here nature works the cure after the proper setting of the injured parts. In compound fracture, on the other hand, the wound extends to the surface, where it comes in contact with the air; and here the operator can never be sure that the most consummate skill will not be neutralised by subsequent putrefaction.

"In the earliest of his published communications, Mr. Lister clearly enunciates, and illustrates by cases of a very impressive character, the scientific principles upon which the antiseptic system rests. He refers to the researches of Pasteur, and shows their bearing upon surgery. He points to the representative fact, then known but unexplained, that when a lung is wounded by a fractured rib, though the blood is copiously mixed with air, no inflammatory disturbance supervenes; while an external wound penetrating the chest, if it remains open, infallibly causes dangerous suppurative pleurisy. In the latter case the blood and serum are decomposed by the microscopic progeny of the germs which enter with the air; in the former case the air is filtered in the bronchial tubes, and all solid particles are arrested. Three years subsequently, this inference of Professor Lister's was shown to be capable of experimental demonstration.

"After enunciating the theoretic views which guided him, he thus expresses himself in his first paper:—

"'Applying these principles to the treatment of compound fracture; bearing in mind that it is from the vitality of the atmospheric particles that all the mischief arises, it appears that all that is requisite is to dress the wound with some material capable of killing these septic germs, provided that any substance can be found reliable for this purpose, yet not too potent as a caustic.'

"This is the thesis to the illustration and defence of which Pro-

fessor Lister has devoted himself for the last thirteen years. His thoughts and practice during this time have been in a state of growth. His insight has been progressive; and the improvement of experimental methods founded on that insight incessant. By contributions of a purely scientific character, which stamp their author as an accomplished experimenter, he has materially augmented our knowledge of the most minute forms of life. The titles of his papers indicate the direction of his labours from time to time; but they give no notion of the difficulties which he has encountered, and successfully overcome. He performs, without dread of evil consequences, the most dangerous operations. He ventures fearlessly upon treatment which, prior to the introduction of his system, would have been regarded as no less than criminal. In the Glasgow Royal Infirmary, when wards adjacent to his had to be abandoned, he operated with success in an atmosphere of deadly infectiveness. Vividly realising the character and habits of the 'invisible enemy' with which he has to cope, his precautions are minute and severe. This demand for exactitude of manipulation has rendered the acceptance of the Antiseptic System slower than it would otherwise have been; but a clear theoretic conception has this value among others: it renders pleasant a minuteness of precaution which would be intolerable were its reasons unknown.

"The operative surgeons of our day have raised their art to the highest pitch of efficiency. Their skill and daring are alike marvellous. Mr. Lister urges an extension of this skill from the operation to the subsequent treatment, contending that every surgeon ought to be so convinced of the greatness of the benefits within his reach, as to be induced to devote to the dressing of wounds the same kind of thought and pains which he now devotes to the planning and execution of an operation. His impressive earnestness; his clearness of exposition; his philosophic grasp of the principles on which his practice is founded—above all his demonstrated success—have borne their natural fruit in securing for him the recognition and esteem of the best intellects of the age."

"In a letter addressed to the writer on the 29th of September, 1880, Professor Helmholtz expresses himself thus:—

"'Professor Lister ist als einer der hervorragendsten Wohlthäter der Menschheit zu betrachten, und als eines der glänzendsten Beispiele, wie segensreich scheinbar minutiöse und abstruse wissenschaftliche Untersuchungen, wie die über die Erzeugung mikroskopischer Organismen, werden können, wenn sie von einem Manne von umfassendem geistigem Gesichtskreise aufgenommen werden.'"

"In a letter dated October 1st, 1880, Professor Du Bois Reymond writes:—

"'The period of bloody warfare through which we passed not long ago, just when Professor Lister's methods were matured enough to be

freely used even on the battlefield, has of course contributed to render his name popular throughout Germany; nay, to make it a household word in many homes. We use the word "listern" as a verb to designate the use of the carbol-spray while bandaging a wound. I do not hesitate to proclaim Lister the greatest benefactor of mankind since Jenner's wonderful discovery—far superior, indeed, to Jackson and Simpson; because, whatever may be the dread of pain and the blessing of being spared it, in Lister's invention health and life itself are concerned, as in hardly any other medical discovery except vaccination. Moreover, the general ideas which have led to Professor Lister's conception stamp his work with a peculiarly scientific character.'"

"In a letter dated from Vevey on the 10th of this month, Professor Klebs, of Prague, himself a distinguished worker in this field, expresses in the strongest terms his admiration of the profound philosophical intuition and practical success of Mr. Lister, as having not only reformed the whole art of Surgery, but given a new impulse to medical science generally. Professor Klebs's interpretation of the opposition encountered for a time by Mr. Lister is worthy of mention. He ascribes it to the high standard attained by British Surgery before the time of Lister. 'The operators,' he says, 'that work under the best hygienic conditions will not feel so acutely as others do the necessity of disinfecting wounds. But the good results of the former British Surgery are now surpassed by the new method, which is accepted at the present time by the whole world.'

"Such testimonies might be multiplied to any extent. The foregoing are the answers received from the only three gentlemen who have been requested to express an opinion as to the merits of Mr. Lister."

A Royal Medal has been awarded to Captain Andrew Noble, late R.A., F.R.S. Captain Noble is joint author with Professor Abel, of the "Researches on Explosives," "Phil. Trans.," 1875, which, in combination with other labours in the same field, procured for Professor Abel the honour of the Royal Medal in 1879. To Professor Abel is due mainly the chemical part of these investigations; to Captain Noble, the mechanical and mathematical part. Each is a complement of the other, but it may be safely affirmed that they could not have been presented to the world in the form in which they appear without the co-operation of Captain Noble's remarkable union of technical knowledge and mastery of mathematical analysis with the chemical science of Professor Abel. His beautiful invention of the Chronoscope, an instrument constructed by him at great cost, by which intervals of time as small as the one-millionth part of a second can be measured, has been of indispensable value in these

researches. He is the author of papers which have been translated into most European languages on subjects of gunnery and gunpowder; he is perhaps the highest authority we possess on the higher branches of artillery science, and the best known on the Continent. His great talents and attainments are not more conspicuous than his singular modesty, and his indefatigable industry. He has been engaged on these subjects about twenty years, having published the first experiments in this country with Navez' electroballistic apparatus, in 1862.

The Rumford Medal has been awarded to Dr. William Huggins, F.R.S. In 1866, a Royal Medal was awarded to Dr. Huggins for his important researches. Since that time he has been continually engaged in prosecuting his investigations and in extending them over a wider range. Thus he has determined the radial component of the velocity of the heavenly bodies relatively to our earth, by means of the alteration of the refrangibility of certain definite kinds of light which they emit, or which are stopped by their atmospheres. The smallness of the alteration corresponding to a relative velocity comparable with the velocity of the earth in its orbit makes the determination a matter of extreme delicacy. But as early as 1868, he had obtained such trustworthy determinations, that he was able to announce before the Royal Society that Sirius was receding from our solar system with a velocity of about 29·94 miles per second.

In a paper presented to the Royal Society in 1872, he has given the results obtained for a large number of stars, and has shown that some are receding and some approaching, and that there seems to be a balance of recession in those parts of the heavens, from which we have reason, from the observed proper motions, which of course can only be transversal, to conclude that the solar system is receding, and a balance in favour of approach in the opposite direction; while yet it does not appear that the motion of the solar system would alone account for the whole of the proper motions of the stars in a radial direction.

The same inquiry was extended to the nebulæ, the spectrum of which consists of bright lines, and in this case it presented greater difficulties. As those nebular lines which appear pretty certainly to be identifiable with hydrogen, are too faint to be employed in the investigation, and the others are not at present identified with those of any known element or compound, he was obliged to avail himself of a coincidence between the brightest nebular line and a line of lead. But as the coincidence is probably merely fortuitous, the results give only the *differences* of approach or recess of different nebulæ. The observations seem to show that, so far as has been observed, the nebulæ are objects of greater fixity as regards motion in space, than the stars.

The other subject to which Dr. Huggins has more particularly

devoted himself of late, is the mapping of the photographic spectra of stars. This was a research of great delicacy, partly on account of the small quantity of light at the disposal of the observer, partly from the great accuracy with which the comparison had to be made with the spectra of known substances, in order that satisfactory conclusions should be deducible as to the presence or absence of such or such substances in the stars. The results obtained, led to a remarkable division of the stars into two great classes, naturally with transition cases, namely, white stars, which showed a group of twelve dark lines belonging, apparently, to the same substance, probably hydrogen, and the group of stars, of which our own sun may be taken as a type.

Besides the researches already mentioned, other papers have been presented by Dr. Huggins to the Royal Society, on the spectra of comets, on the spectrum of Uranus, and in particular one in which he showed that it was possible to detect the heat of the stars, and has given the results obtained for several.

The Davy Medal has been awarded to Professor Charles Friedel, Member of the Institute of France.

From 1856 to the present time, the investigations of M. Charles Friedel, ranging over widely remote fields of chemical inquiry, have been continuous, numerous, and important. Mineralogical, theoretical, and general chemistry are indebted to him for many valuable contributions; but it is in the department of so-called organic chemistry that he has more especially laboured; and herein he has done much to assist in breaking down the barriers at one time regarded as impassably isolating the chemistry of carbon compounds.

Among the subjects of M. Friedel's successful work may be mentioned more particularly the chemistry of the 3-carbon family of organic bodies, to which belong propionic acid, lactic acid, glycerine, propylene, and acetone. The establishment of the constitution of lactic acid and of acetone, with the determination of the relationships to one another of the various, and in many cases isomeric, members of this large family, constituted for a long time one of the most fiercely contested as it was, and is, one of the most fundamental problems of organic chemistry. In the labours effecting the satisfactory solution of this problem, M. Friedel bore a large share.

Passing to another branch of investigation, M. Friedel, partly by himself, but largely in conjunction in some parts of the work with Mr. J. M. Crafts, and in other parts with M. A. Ladenburg, made out, or confirmed in a very striking manner, the analogy subsisting between the modes of combination of carbon and of silicon, the most characteristic elements of the organic and inorganic kingdoms respectively.

To mention but one more subject of M. Friedel's research, he has, in conjunction with Mr. J. M. Crafts, made out and defined a simple

method of wide application for effecting the synthesis of organic compounds. This method consists in bringing together a hydrocarbon and an organic chloride in presence of chloride of aluminum, whereby the residues of the two compounds enter into combination to form a more complex, frequently a highly complex body. Independently of its utility, this process of synthesis is of remarkable interest from the part taken in it by the chloride of aluminum, which, though essential to the reaction, is found unaltered at the end, and seems to act by suffering continuously, little by little, a correlative transformation and regeneration.

The Statutes relating to the election of Council and Officers were then read, and Mr. Francis Galton and Mr. Sorby having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Alexander William Williamson, Ph.D.

Other Members of the Council.

William Henry Barlow, Pres. Inst. C.E.; Rev. Professor Thomas George Bonney, M.A.; George Busk, F.L.S.; Right Hon. Sir Richard Assheton Cross, D.C.L., LL.D.; Edwin Dunkin, V.P.R.A.S.; Alexander John Ellis, B.A.; Thomas Archer Hirst, Ph.D.; William Huggins, D.C.L., LL.D.; Professor John Marshall, F.R.C.S.; Professor Daniel Oliver, F.L.S.; Professor Alfred Newton, M.A., Pres. C.P.S.; Professor William Odling, M.B., V.P.C.S.; Henry Tibbats Stainton, F.G.S.; Sir James Paget, Bart., D.C.L.; William Henry Perkin, Sec. C.S.; Lieut.-Gen. Richard Strachey, R.E., C.S.I.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Com-pounders.	£4 yearly.	£3 yearly.	Total.
Dec. 1, 1879 ..	4	48	241	236	11	540
Since Elected ..			+ 3	+ 1	+ 14	+ 18
Since Deceased..		− 1	− 8	− 11		− 20
Since Withdrawn				− 1		− 1
Nov. 30, 1880 ..	4	47	236	225	25	537

Statement of Receipts and Expenditure from November 20, 1879, to November 26, 1880.

Receipts					£	s.	d.
Annual Contributions, 231 at £4	£924				999	0	0
" 25 at £3	75						
Admission Fees					20	0	0
Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions					336	0	0
Compositions					120	0	0
Rents					201	9	7
Dividends (exclusive of Trust Funds)					1,146	4	2
" on Jodrell Fund					151	18	4
Interest on Mortgage Loan					586	13	10
Sale of Transactions and Proceedings					789	11	6
Donation from J. S. Budgett, Esq.					100	0	0
Sale of land at Acton					495	17	9
Petty Repayments					6	8	9
Deposit at Bankers' withdrawn					1,800	0	0
Interest on Do.					8	9	6
Balance at Bank, Dec., 1879		1,222	12	0	1,264	6	8
On hand		41	14	8			
					£8,026	0	1

Expenditure	£	s.	d.	£	s.	d.
Salaries and Wages				1,089	11	0
The Library Catalogue				107	8	6
Books for the Library				169	10	0
Printing Transactions, Part III. 1879, Part I. 1880, and Separate Copies to Authors and Publisher	594	4	11			
Ditto Proceedings, Nos. 198–206	384	13	11			
Ditto Miscellaneous	53	4	1	2,121	14	7
Paper for Transactions and Proceedings	364	4	6			
Binding ditto	64	14	6			
Engraving and Lithography	660	12	8			
Soirée and Reception Expenses				78	9	4
Coal, Lighting, &c.	86	3	6			
Office Expenses	24	0	8			
House Expenses	238	7	6			
Tea Expenses	16	15	4			
Fire Insurance	41	15	0	538	5	11
Taxes	33	5	10			
Advertising	22	2	0			
Postage, Parcels, and Petty Charges	36	2	4			
Miscellaneous Expenses	39	13	9			
Purchase of Land at Acton				1,160	0	0
Roadmaking, Agency and Survey of Acton Estate				499	7	11
Agency and Survey of Mablethorpe Estate				20	18	0
Law Charges				77	13	2
Purchase of £952 12*s.* 3*d.* Reduced 3 per Cent. Annuities				925	4	6
Gassiot Trust, Loan repaid				200	0	0
Transferred to Fee Reduction Fund. Donation of G. Palmer, Esq., M.P.				200	0	0
				£7,188	2	11

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	240	16	8			
Handley Fund ,,	177	5	3			
Rumford Fund ,,	68	4	8			
Wintringham Fund ,,	35	5	0	566	12	8
Copley Medal Fund ,,	9	17	6			
Davy Medal Fund ,,	32	4	10			
Croonian Lecture Fund—Rent from College of Physicians	2	18	9			
				£8,592	12	9

	£	s.	d.	£	s.	d.
Donation Fund	130	0	0			
Davy Medal Fund	32	11	0			
Wintringham Fund	35	5	0	209	4	9
Copley Medal Fund	4	10	0			
Bakerian Lecture	4	0	0			
Croonian Lecture	2	18	9			
Balance at Bank				1,178	9	7
Balances on hand, Catalogue Account	4	14	3	16	15	6
,, ,, Petty Cash	12	1	3			
				£8,592	12	9

JOHN EVANS,

Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (about 33 acres), £152 per annum (Contract for Sale sealed).
Fee Farm near Lewes, Sussex, rent £19 4*s*. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £484 5*s*. 2*d*. in 1879-80).
£21,000 Reduced 3 per Cent. Annuities.
£15,000 Mortgage Loan, 4 per Cent.
£16,588 6*s*. 2*d*. Consolidated Bank Annuities.
£200 " " —The Gassiot Trust.
£403 9*s*. 8*d*. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£11,511 6*s*. New Threes { £6,328 11*s*. 2*d*. Scientific Relief Fund.
5,182 14*s*. 10*d*. Jodrell Fund. }
£667 5*s*. 6*d*. India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.
£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.
£100 Metropolitan 3½ per Cent. Stock.—Scientific Relief Fund.
£1,300 " " —Fee Reduction Fund.
£7,000 London and North Western Railway 4 per Cent. Debentures. } " "
Two Hundred Shares in the Whitworth Land Company, Limited. } " "

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct, and we find that the Balance at the Bankers' is £1,178 9*s*. 7*d*.

W. SPOTTISWOODE.
T. H. HUXLEY.
GEO. BUSK.
H. CLERK.
W. H. PERKIN.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct, and we find that the Balance at the Bankers' is £1,178 9*s*. 7*d*.

J. T. BOILEAU.
FREDK. CURREY.
WARREN DE LA RUE.
ROBERT HUDSON.
G. MATTHEY.

Trust Funds. 1880.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,328	11	2
Metropolitan 3½ Consols	100	0	0
	£6,428	11	2

Dr. *Cr.*

	£	s.	d.		£	s.	d.
To Balance	98	19	7	By Grants	190	0	0
„ Dividends	191	2	1	„ Balance	100	1	8
	£290	1	8		£290	1	8

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

The Trevelyan Bequest.

£1,396 Great Northern Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	219	5	4	By Grants	130	0	0
„ Dividends	240	16	8	„ Balance	330	2	0
	£460	2	0		£460	2	0

Rumford Fund.

£2,322 19*s*. Consols.

	£	s.	d.		£	s.	d.
To Balance, 1879	68	4	8	By Balance	136	9	4
„ „ 1880	68	4	8				
	£136	9	4		£136	9	4

Bakerian and Copley Medal Fund.

£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent.

	£	s.	d.		£	s.	d.
To Balance	75	1	6	By Gold Medal	4	10	0
„ Dividends	9	17	6	„ Bakerian Lecture	4	0	0
				„ Balance	76	9	0
	£84	19	0		£84	19	0

Wintringham Fund.

£1,200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1879	35	5	0	By Payment to Foundling Hospital, 1880	35	5	0
„ Dividends, 1880	35	5	0	„ Balance	35	5	0
	£70	10	0		£70	10	0

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1879	2	18	9	By Croonian Lecture	2	18	9
„ One-fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	18	9	„ Balance	2	18	9
	£5	17	6		£5	17	6

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	139	11	2	By Gold Medal	32	11	0
„ Dividends	32	4	10	„ Balance	139	5	0
	£171	16	0		£171	16	0

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£	s.	d.		£	s.	d.
To Balance	291	0	0	By 2 Italian Bonds bought	203	5	0
„ Dividends	499	12	2	„ £200 Consols bought	194	5	0
„ Bonds drawn	234	10	0	„ Payments to Kew Committee	496	13	8
				„ Balance	130	18	6
	£1,025	2	2		£1,025	2	2

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1880	151	18	4	By transferred to Royal Society General Account	151	18	4

The Handley Fund.

£6,047 7*s.* 9*d.* Reduced 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance, 1879	177	12	10	Transferred to Royal Society General Account	354	18	1
„ Dividends, 1880	177	5	3				
	£354	18	1		£354	18	1

Fee Reduction Fund.

£1,300 Metropolitan Consols 3½ per Cent.
£7,000 London and North Western Railway 4 per Cent. Debentures.
Two Hundred Shares in the Whitworth Land Company, Limited.

	£	s.	d.		£	s.	d
To Balance (1879)	996	6	6	By Transferred to Royal Society General Account (1879)	161	0	0
„ Donations	555	0	0	„ Transferred to Royal Society General Account (1880)	175	0	0
„ Dividends	307	9	7	„ Purchase of £1,000 London and North Western Railway Company's 4 per Cent. Debenture Stock	1,103	2	3
				„ Purchase of £314 15*s.* 7*d.* Metropolitan Consols 3½ per Cent.	331	0	9
				„ Balance	88	13	1
	£1,858	16	1		£1,858	16	1

Account of Grants from the Donation Fund in 1879–80.

Professor W. C. Williamson, to aid in continuing his Investigations of the Fossil Plants of the Coal Measures	£30	0	0
Dr. Anton Dohrn, to aid in the publication of Monographs on the "Fauna and Flora of the Gulf of Naples, and adjacent parts of the Mediterranean"	£100	0	0
	£130	0	0

Account of the appropriation of the sum of £1,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from Vol. XXIX, p. 440.)

1880.

1. J. N. Lockyer, for continuation of Researches on the Solar Spectrum	£200
2. Professors Thorpe and Rücker, for continuation of the Comparison of the Air and Mercurial Thermometers	75
3. A. M. Worthington, for Researches on the Tension of Liquid Surfaces	10
4. W. G. Adams and E. B. Sargeant, for the Determination of the Ratio of the Lateral Contraction to the Longitudinal Extension in a Cylindrical Bar of a Homogeneous Elastic Solid subjected to simple Longitudinal Stress	100
5. W. Crookes, for continuation of Researches on Molecular Physics in High Vacua	300
6. W. C. Roberts, for assistance in Conducting Researches on the Passage of Molten Metals and Alloys through Capillary Tubes	25
7. Professor Rupert Jones, for continuing the Illustration of Fossil Entromostraca	25
8. G. E. Dobson, for Investigation of the Natural History of the Mammalian Order Insectivora, with the view of publishing as complete a Monograph as possible of this comparatively little studied order of Mammals, in which full descriptions, with the Anatomy and Geographical Distribution of every Species, will be given	50
Carried forward................	£785

Brought forward................	£785
9. Baron Ettingshausen, for the Investigation of the Eocene Flora of Alum Bay, Bournemouth, and other places	50
10. E. A. Schäfer, for Payment of an Assistant in continuing his Histological and Embryological Investigations	50
11. C. F. Cross, for the cost of a Balance and Materials to be employed in a Research on Rehydration of Metallic Oxides	20
	£905

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance on hand, Nov. 30, 1879......................	1,022	9	11	By Appropriations, as above	905	0	0
Grant from Treasury, 1880	1,000	0	0	Printing, Postage, and Advertising	4	14	0
Interest	5	2	8	Balance on hand, Dec. 1, 1879	1117	18	2
	£2,027	12	2		£2,027	12	2

Account of Appropriations from the Government Fund of £4,000 made by the Lords of the Committee of Council on Education, on the recommendation of the Council of the Royal Society.

G. J. Symons, for a Computation of the Mean Annual Rainfall at all known Rainfall Stations in the British Isles at which the requisite Data exist (α) during the ten years 1867–76 (β) and also during the ten years 1870–79; to thoroughly discuss the same, and to prepare a Monograph thereupon	£120
R. H. M. Bosanquet, for the cost of an Engine with Clock, Bellows, and other appliances to be employed in the Solution of various Problems in Acoustics, the repetition and examination of König's Experiments, the Determination of Absolute Pitch, and the transformation of Sound into Periodic Electric Currents ...	152
Carried forward................	£272

Brought forward................ £272

A. Mallock, for the Construction of a Room and Foundation for the Diffraction Grating Ruling Machine, and for Water Power.. 50

G. Gore, for (1) further Investigation of the Thermoelectric Properties of Liquids; (2) Completion of a Research into the Phenomena of the Capillary Electroscope; and (probably) (3) Examination of the Effects of Electric Currents in Friction.... 100

Dr. Hopkinson, for continuing Experiments on the Residual Charge of the Leyden Jar, on Specific Inductive Capacity, and for other Electrostatic Experiments 150

E. Neison, for continuation of Computations in the Lunar Theory 75

D. Gill, for the Payment of Computers to aid in the Reduction of Heliometric Observations made by him at the Cape of Good Hope 75

J. Kerr, for continuation of Electro-Optic and Magneto-Optic Researches.. 100

J. Glaisher, towards the Expense of Printing the Factor Table of the Fourth Million.................................. 150

Professor R. Grant, for the expense of Printing a Catalogue of the Mean Places of 6,350 Stars, based on Observations made at the Glasgow Observatory.............................. 300

Professors Liveing and Dewar, for continuation of Experiments in relation to Spectroscopic Investigations.............. 200

A. Buchan, for further Aid in the Construction of Isobaric Charts for the United Kingdom.......................... 100

T. Stevenson, for averaging and discussing a Register of 23 years' Observations of Temperature of the Barometer and Thermometer, and of Rain, kept at Rothesay 50

A. Tribe, for Researches into the Distribution of Radicals in Metals suspended in Electrolytes in the act of Electrolysis; and Analogies (or otherwise) between Phenomena in Dielectric and Electrolytic Media.. 50

J. E. H. Gordon, for continuation of Experimental Measurements of the Specific Inductive Capacity of Dielectrics........ 73

B. Stewart, for an Investigation of Inequalities of Short Period in a Series of Observations 60

H. Tomlinson, for continuation of Researches into the Effect of Strain and Pressure on Matter when under the Influences of the Forces of Heat, Electricity, Magnetism, and Chemical Affinity 100

Samuel Allport, for an accurate Investigation of the Pheno-

Carried forward................ £1,905

Brought forward................ £1,905

mena of Contact Metamorphism, especially that produced by the Intrusion of Granite among Sedimentary Deposits............ 100

W. K. Parker, for assistance in continuation of Researches on the Morphology of the Vertebrate Skeleton, and the Relations of the Nervous to the Skeletal Structure 300

Professor Heddle, for continuation of a Research connected with the Scientific Mineralogy and Geognosy of Scotland 150

H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association 100

D. Mackintosh, for an Examination of the Southerly Extension of Northern Erratics into the Valleys and up the Hill Slopes of North Wales, with a particular reference to the Extreme Height by the great Glacial Submergence 15

Nicholson and Etheridge, for Further Assistance towards the Publication of the Third Fasciculus of their "Monograph of the Silurian Fossils of Girvan, Ayrshire" 75

R. Etheridge, jun., for the Cost of Illustrations of New or little known British and Australian Palæozoic Invertebrate Fossils.. 25

Major Godwin Austen (on behalf of British Association Committee), for Investigation of the Natural History of the Island of Socotra 125

J. M. Crombie, for aid in Researches on the Origin and Structure of the Vegetative and Reproductive Organs of Lichens, with special reference to the recent Theory that Lichens are not Autonomous Plants 100

B. T. Lowne, for further Researches on the Structure, Development, and Functions of the Eyes of Invertebrates 50

H. G. Seeley, for completion of Researches into the Structure, Affinities, and Classification of the extinct Reptilia and allied Animals 80

H. Woodward, for continuation of Work on the Fossil Crustacea, especially with reference to the Trilobita and other extinct Forms, and their publication by the Palæontographical Society 75

C. Lapworth, for Assistance in continuing Studies of the Graptolites, and of the Silurian Rocks of Britain 70

Rev. J. F. Blake, for travelling and other expenses in continuing a Correlation of the Upper Jurassic Deposits of England with those of the Continent 100

Professor Schorlemmer, for continuation of Researches into (1) Aurin; (2) the Normal Paraffins; (3) Suberone 200

Carried forward............... £3,470

Brought forward................	£3,470
C. G. Williams, (1) for an Investigation of the Chemical History and Physiological Action of β Lutidine; and (2) to continue Researches on Emeralds and Beryls	50
C. R. A. Wright, for continuation of Investigations on the determination of Chemical Affinity in Terms of Electrical Magnitude	150
Dr. Dupré, for the cost of Apparatus and Materials in carrying out experiments with a Gravimetric Method in estimating extremely small quantities of Carbon, and its application to the Examination of Potable Waters............................	75
J. H. Collins, for continuation of Chemical, Mineralogical, Microscopical, Stratigraphic Observations on and Investigations of the Rocks of Cornwall	30
W. N. Hartley, for continuation of Researches on the Action of Organic Substances on the Ultra-violet Rays of the Spectrum	100
Professor A. H. Church, for continuation of Researches in Plant Chemistry	50
	£3,925
Administrative Expenses	75
	£4,000

Report of the Kew Committee for the Year ending October 31, 1880.

The operations at the Kew Observatory in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is now constituted as follows:

General Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue, *Vice-Chairman.*
Prof. W. G. Adams.
Capt. F. Evans, C.B.
Prof. G. C. Foster.
Mr. F. Galton.
Lieut.-Gen. Sir J. H. Lefroy, K.C.M.G.
Vice-Adm. Sir G. H. Richards.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-Gen. R. Strachey, C.S.I.
Mr. E. Walker.

The work at the Observatory may be considered under seven sections:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with either of the above departments.
5th. Verification of instruments.
6th. Aid to other Observatories.
7th. Miscellaneous.

I. Magnetic Observations.

No change has been made in the Magnetographs, which have worked continuously during the year. The curves have recently indicated the approach of a more disturbed period than has occurred for some few years, and a magnetic storm of considerable intensity was registered from August 11th to 15th.

Owing to the gradual secular change of declination, the distance between the dots of light upon the cylinder of the magnetometer had become too small for satisfactory registration, and it was found necessary to readjust the instrument by a displacement of its zero. From a similar cause it was also found necessary to readjust the balance of the vertical force magnetometer.

The scale values of all the instruments were re-determined in January, in accordance with the usual practice.

The monthly observations with the absolute instruments have been made regularly, and the results are given in the tables forming Appendix I of this Report.

The Sub-Committee, appointed to consider the best means of utilising the records of the magnetographs, as mentioned in the Report for 1878, reported that it was unadvisable, in their opinion, to proceed with the regular tabulation of the curves, and suggested that attention should rather be directed to their comparison with synchronous curves, taken at other magnetic Observatories in different parts of the globe, in order to ascertain whether similar disturbances occur at these several stations, and at what time intervals; with a view to the development of the theory of magnetic disturbance.

In order to carry out this scheme, a circular, inviting co-operation on the part of observers provided with magnetographs of the Kew pattern, was issued to the Directors of the following Observatories:—Batavia, Bombay, Brussels, Coimbra, Colaba, Lisbon, Mauritius, Melbourne, Potsdam, St. Petersburg (Pawlowsk), San Fernando, Stonyhurst, Utrecht, Vienna, and Zi-Ka-Wei. Replies favourable to the project were received from all those whose instruments were working under satisfactory circumstances.

An examination of the records for the year 1879 indicated the month of March as that most suitable for the purpose of the comparison. Accordingly, a further request for copies of the declination curves for that month was issued, and, in response, they have at present been received from:—

Coimbra, Colaba, Lisbon, Melbourne, St. Petersburg, Stonyhurst, Vienna, and Utrecht.

The comparison of these magnetic curves has been undertaken by Professor W. Grylls Adams, who has already communicated to the Swansea Meeting of the British Association a preliminary account of the principal facts which have as yet come to light. The discussion, which is still in progress, cannot be completed until data from the more distant stations, as well as the horizontal and vertical force curves from all stations for the same month, have arrived.

The Observatory has also received curves from several of the foreign Observatories, showing the variations recorded by their instruments during the progress of the magnetic storm already referred to.

By the kindness of Professor G. Carey Foster, some experiments were made at the laboratory of University College, London, with a view to determine whether the magnetisation of dip-needles could be conveniently effected by means of a coil of wire conveying an electric current, thereby avoiding certain defects due to their magnetisation by bars, after the ordinary method. The results of these experiments proved that the requisite magnetic intensity could be easily imparted in the way referred to.

At the request of Dr. E. Van Rijckevorsel, observations have been made with dip-needles constructed of nickel, and also with others of steel nickel plated in order to avoid the injurious effects of rust. The nickel plating proved successful; but it was found impossible to impart a sufficient degree of magnetism to the nickel needles to allow of their giving reliable results.

The magnetic instruments have been studied, and a knowledge of their manipulation obtained by Dr. Chistoni and Dr. Harris.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professor W. G. Adams, Mr. Adie, Professor Barrett, Messrs. Barker and Son, Mr. Casella, Professor G. C. Foster, Mr. J. E. H. Gordon, Mons. Marié-Davy, Dr. Rijckevorsel, and Professor Balfour Stewart.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of Horizontal Intensity	25
„ Dip	164
„ Absolute Declination	37

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively, of atmospheric pressure, temperature, humidity, wind (direction and velocity), and rain have been maintained in regular operation throughout the year.

New fume pipes have been fitted over the thermograph and electrograph to carry off the products of combustion of the gas more efficiently than the old ones, which had become much corroded.

The standard eye observations made five times daily, for the control of the automatic records, have been duly registered through the year, together with the additional daily observations at 0 h. 45 m. P.M. in connexion with the Washington synchronous system, and at 6 h. 45 m. P.M., for the second synchronous system organized by M. Mascart, Directeur du Bureau Central Météorologique, Paris.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted weekly to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	1934
„ dry and wet thermometers	6546
„ maximum and minimum thermometers	2196

Readings of radiation thermometers		848
„	rain and evaporation gauges	1184
Cloud and weather observations		2300
Measurements of barograph curves		9477
„	dry bulb thermograph curves	9513
„	wet bulb thermograph curves	9405
„	wind (direction and velocity)	18940
„	rainfall curves	639
„	sunshine traces	2094

In compliance with a request made by the Meteorological Council to the Kew Committee, the Observatories at Aberdeen, Armagh, Falmouth, Glasgow, Oxford (Radcliffe), Stonyhurst, and Valencia, have been visited as usual and their instruments inspected by Mr. Whipple during his vacation.

With the concurrence of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by "The Times," "The Illustrated London News," and "The Torquay Directory," and meteorological data have been supplied to the editor of "Symons' Monthly Meteorological Magazine," the Secretary of the Institute of Mining Engineers, Messrs. Anderson, Buchan, Eaton, Greaves, McDonald, Rowland, Wragge, and others.

Electrograph.—This instrument has been in continuous action through the year.

During the severe frost of last winter it was found necessary to heat the water flowing through the discharge pipe by means of a spirit lamp, suspended from the collector. This precaution enabled the records to be maintained throughout the year, with very few interruptions due to frost.

In August the instrument was dismounted, and a fresh supply of acid placed in the jar, the charge-keeping properties of which had become slightly deteriorated.

Some experiments have been made with a view of determining the effect of the interposition of an air condenser between the collector and the electrometer, in reducing the extent and rapidity of the electrical changes registered by the instrument under certain atmospheric conditions. These experiments are still in progress.

No steps have yet been taken as to the discussion of the seven years' curves now in store, but suggestions as to the means of dealing with them are under consideration.

The self-recording instruments, with their attendant photographic processes and methods of tabulation, have been studied by Professor C. Niven, who has succeeded the late Professor D. Thomson in the charge of the Aberdeen Observatory; by Dr. Chistoni, of the Roman Observatory; and by M. Perrotin, Director of the Nice Observatory.

The spare barograph, thermograph, and Beckley rain gauge, the property of the Meteorological Council, formerly deposited at the Observatory, having been lent by the Council to the Radcliffe Trustees, were set up at their Observatory in Oxford at the beginning of the year.

With a view to prevent certain failures occasionally taking place in the photographic system of registration, which are attributed to chemical action in the wax used in the preparation of the paper, it has been considered desirable to introduce in part of the work, by way of experiment, a new process devised by Captain Abney, R.E., F.R.S., in which unwaxed paper is employed.

At the request of Admiral Mouchez, Directeur de l'Observatoire National, Paris, a set of copies of the autographic records, together with descriptions of the instruments and other particulars respecting the Observatory, has been forwarded to the Museum recently established in that Institution.

III. Solar Observations.

The preliminary reductions of the measurements of the Kew solar negatives having been completed in January last, a re-examination of the pictures was made with the object of classifying the spots according to a scale of figure and magnitude; this being now terminated, Mr. McLaughlin is engaged assisting Mr. Marth in the reduction to heliocentric elements of the pictures from January, 1864, to April, 1872.

These operations have all been conducted under the direction and at the expense of Mr. De La Rue.

The eye observations of the sun, after the method of Hofrath Schwabe, as described in the Report for 1872, have been made on 246 days, in order to maintain for the present the continuity of the Kew records of sun-spots. The sun's surface was observed to be free from spots on 27 of those days.

A catalogue of the whole of the solar photographs taken at Kew during a decade 1862 to 1872, has been prepared and forwarded to the Solar Committee of the Science and Art Department.

At the request of the Council of the Royal Astronomical Society, the valuable collection of MSS. containing the memorable series of sun-spot observations made by Hofrath Schwabe, of Dessau, during the years 1825 to 1867, which had been deposited in the Library of the Observatory, the first volumes since 1865, was transferred to the Society's Library at Burlington House, London. In order, however, to render the collection of sun-spot observations at Kew as complete as possible, and to prevent the total loss of the observations in case of fire, the Committee voted the sum of £90 to defray the cost of making a complete copy of the solar drawings.

This was accordingly done, and accurate tracings made of every one of Schwabe's drawings. These were pasted into blank books, and any important notes were transcribed at the same time.

The Observatory, therefore, now possesses a complete record of the condition of the sun's surface, extending from November, 1825, to the present date.

The work was performed by the members of the Observatory staff, in extra hours.

Transit Observations.—Ninety observations have been made of sun-transits, for the purpose of obtaining correct local time at the Observatory: 102 clock and chronometer comparisons have also been made.

Sunshine Recorder.—The Campbell sunshine recorder, described in the Report for 1875, continues in action, and the improved form of the instrument, giving a separate record for every day of the duration of sunshine, has been regularly worked throughout the year, and its curves tabulated. In April last, the new pattern of card-holder, devised by Professor Stokes ("Quarterly Journal Met. Soc.," vol. vi, p. 83) was substituted for that previously employed, in order that the records produced by the instrument might be in conformity with those obtained from the other stations of the Meteorological Council. Since that date both cards and tabulations have been transmitted regularly to the Meteorological Office, copies, however, being retained in the Observatory for reference.

A similar sunshine recorder has been constructed for the Melbourne Observatory, and, after trial and adjustment at Kew, was transmitted together with a set of pattern-cards, through the Crown Agents to Mr. Ellery.

IV. Experimental Work.

Winstanley's Recording Radiograph.—This instrument, designed by Mr. D. Winstanley, as described in "Engineering," vol. xxx, p. 316, for the purpose of registering continuously the amount of radiation from the sky, by mechanical means, upon a sheet of blackened paper, has been erected on the roof of the Observatory since the beginning of August.

Its indications, which were procured for some weeks, showed it to be a much more delicate appliance than the sunshine recorder or the black bulb thermometer, being affected by changes of radiation from the sky, which take place both at night and when the sky is clouded, as well as when the sun is shining. No use has, however, yet been made of its curves, mainly on account of the difficulty of determining a scale value for them.

Wind Component Integrator.—This instrument, owing to the causes referred to in last Report, was not kept in action after that date, and in December it was dismounted. It has since been deposited again in

the Loan Collection of Scientific Apparatus at South Kensington, the costs attendant on its trial at Kew having been defrayed by the Meteorological Council.

Photo-nephoscope.—This instrument is still in the hands of Captain Abney, R.E., but experiments have been made with several other forms of nephoscope, and also with a new cloud-camera, designed by the Superintendent.

Exposure of Thermometers.—Experiments have been continued throughout the year at the Observatory, with the view of determining the relative merits of different patterns of thermometer screens. For this purpose, there have been erected on the lawn a Stevenson's screen, of the ordinary pattern, and a large wooden cage, containing a Wild's screen, of the pattern employed in Russia. Each of these screens contains a dry and a wet bulb thermometer, and a maximum and minimum, all of which are read daily, at 9 A.M. and 9 P.M., their indications being compared with those of the thermograph at the same hours. A third portable metal screen, designed by Mr. De La Rue for use on board Light-ships, which contains a dry bulb thermometer only, is also carried into the open air by the observer, and read at the same time as the fixed instruments.

The cost of these experiments is borne by the Meteorological Council.

Glycerine Barometer.—This instrument, devised and erected by Mr. Jordan, as mentioned in last year's Report, has been in successful operation throughout the year, and, in compliance with the request of the inventor, has been continuously observed in conjunction with the mercurial barometer five times daily. In April last, with a view to the more complete removal of the minute quantity of air which had adhered to the sides of the tube at the time of filling, and had since risen at intervals into the vacuum, air pressure was applied to the lower surface of the column by means of a force pump, and the glycerine driven up to the top of the tube. The small bubble of air was then expelled through the stoppered aperture, its place being filled by a drop of the glycerine from the cup.

A complete description of the instrument, by Mr. Jordan, was read before the Royal Society, on January 22nd, and has been printed in their "Proceedings," vol. xxx, p. 105. As a preparatory step towards the discussion of the observations made with the instrument, Mr. Jordan has computed a table for the reduction of its readings to a temperature of 32° F., the mean coefficient of expansion of glycerine having been determined by Professor A. W. Reinold to be ·000303 for 1° F. between 32° and 212°. The value of the glycerine barometer as an instrument of precision cannot be determined until the observations now in process of reduction by Mr. Jordan have been completed. Meanwhile the Committee have decided to continue the periodical readings, and to make several separate series of readings, at frequent

intervals, during periods of atmospheric disturbance, so as to determine its relative degree of sensibility as compared with ordinary mercurial instruments.

De La Rue Evaporation Gauge.—The Vice-Chairman of the Committee has devised a small evaporation gauge, by means of which the water given off from a continually-wetted sheet of vegetable parchment is measured daily. Two of these instruments, constructed by Messrs. Negretti and Zambra, were set up at Kew, and their indications noted every day, at 10 A.M., together with those of a Piché Evaporimètre, until the end of July, when, at the request of the Meteorological Council, they were transferred to the care of Mr. Shaw, who is at present engaged at Cambridge in an experimental investigation on hygrometry.

De La Rue Anemograph.—The electrical attachment to this instrument having been successfully completed after a somewhat lengthy series of experiments, its registrations were discontinued and the instrument was partially dismounted, in order to allow of its vane being used for certain experiments now in progress with regard to the working of air-meters.

Air Thermometer.—The construction of the Standard Air Thermometer is still delayed, Professors Thorpe and Rücker not having yet completed their comparisons between the mercurial and air thermometers.

By the kindness of Professor H. A. Rowland, of the Johns Hopkins University, Baltimore, U.S.A., the Committee has had the opportunity afforded it of comparing with a number of Kew standards, one of the thermometers which Professor Rowland has employed in his researches on the deviation of the mercurial from the air thermometer. The instrument is that—Baudin, No. 6166—which Dr. Joule ("Proc. Amer. Acad. Arts and Sciences, 1880") compared with the instrument he used in his determination of the mechanical equivalent of heat ("Phil. Trans., 1878"). Professor Rowland has kindly promised to present the Committee with another of his standards, which has been compared with his air thermometer throughout a greater range of scale than the present instrument.

V. Verification of Instruments.

The following magnetic instruments have been verified, and their constants have been determined:—

A Unifilar, by Gibson, for Elliott Brothers.
Four Dip-circles, by Casella.
A pair of Dipping-needles for Elliott Brothers.
Three Dipping-needles for Dr. E. Van Rijckevorsel.
Two Magnetograph-needles for M. Dechevrens, Zi-Ka-Wei.
An Azimuth Compass for Barker and Son.

There have also been purchased on commission and verified:—

A Dip-circle for Dr. Mielberg, Tiflis.

A Dip-circle for the Russian Expedition to the Mouth of the Lena.

Two Magnetograph-needles for Dr. Wild, St. Petersburg.

There has been a satisfactory increase in the number of meteorological instruments verified, which was as follows:—

Barometers, Standard	47
" Marine and Station	156
Aneroids	21
Total	224

Thermometers, ordinary Meteorological	1487
" Standard	94
" Mountain	68
" Clinical	3638
" Solar radiation	57
Total	5344

Besides these, 22 Deep-sea Thermometers have been tested, 14 of which were subjected in the hydraulic press, without injury, to strains exceeding three and a half tons on the square inch, and 165 Thermometers have been compared at the freezing-point of mercury, making a total of 5,531 for the year.

Duplicate copies of corrections have been supplied in 20 cases.

A special set of Standard Thermometers has been constructed for the Bureau International des Poids et Mesures, at Paris.

Seventeen Standard Thermometers have also been calibrated and divided, and supplied to societies and individuals during the year.

Three Metre Scales have been divided on glass for the University College Laboratory.

The following miscellaneous instruments have also been verified:—

Hydrometers	10
Anemometers	12
Rain Gauges	13
Sextants	5
Theodolites	4
Cathetometer Scales	2

There are at present in the Observatory undergoing verification, 40 Barometers, 50 Thermometers, 1 Hydrometer, and 2 Anemometers.

Anemometer Testing.—The Committee have had before them the question of the desirability of erecting a suitable apparatus for the

testing of Anemometers and Air-meters; but in the opinion of Dr. Robinson it will be better to postpone its erection for a time. Meanwhile these instruments, temporarily erected on the roof, are compared directly with the Standard Anemograph, and tables of corrections supplied to reduce their readings to the same scale of velocities as that indicated by the latter instrument.

The experiments made in 1874, and described in the Report for that year, to determine by means of a "steam-circus" at the Crystal Palace, the true value of Robinson's factors for Anemometers at different velocities, are under discussion by Professor G. G. Stokes, F.R.S., and have been found to afford valuable results. A paper, which he intends to communicate on the subject to the Royal Society, is nearly ready.

Experiments have been made with one of M. Hagemann's Anemometers ("Quart. Jour. Met. Soc.," vol. v, p. 203), designed for use at sea, the results being submitted to the Meteorological Council.

A Bridled Anemometer, designed by Mr. F. Galton, has also been tried.

The Galton Thermometer-tester has had a new water-heater fitted to it, and has besides undergone thorough repair and renovation.

The Winchester Observatory of the Yale College, U.S.A., having recently established a department on the Kew system, for the verification of thermometers, Professor Newton, Secretary of the Institution, visited our Observatory, studied the methods employed for comparing thermometers, and procured copies of the various forms and certificates used in the work.

The Sextant-testing apparatus has been improved during the year by the substitution of reticules, photographed on glass, for the glass threads in the focus of the collimators. The latter, by their breakage, rendered frequent re-adjustment of the instrument necessary.

Standard Barometers.—Numerous comparisons have been made during the year between the two Welsh Standard Barometers, the old Royal Society Standard (which it is found cannot without risk of derangement be returned to Burlington House), and Newman, No. 34, the working Standard of the Observatory.

Arrangements have been made by means of which the latter may, when desired, be read by the cathetometer, as well as by its own scale, the correct value of which has also been re-determined.

VI. Aid to Observatories.

Waxed Papers, &c., supplied.—Waxed paper has been supplied to the following Observatories:—

Batavia, Colaba, Glasgow, Lisbon, Montsouris (Paris), Mauritius, Oxford (Radcliffe), and Utrecht.

Anemograph Sheets have also been sent to the Madras Observatory, and Mauritius, and

Blank Forms for the entry of magnetic observations to Professor Young, Princetown, U.S.A.

VII. Miscellaneous.

Loan Exhibition.—With the exception of the Hodgkinson's Actinometer and the three instruments mentioned in the 1878 Report, the instruments specified in the Report for 1876 still remain in charge of the Science and Art Department, South Kensington.

At the request of the Secretary of the Royal Society several sets of comparisons have been made between the Hodgkinson's Actinometers, the property of the Royal Society, and a similar instrument sent home from India by Mr. Hennessey, F.R.S., who has observed with it in that country.

International Comparison of Standards.—The Committee received an application from the Secretaries of the Comité International de Méteorologie inviting them to assist in the suggested scheme of an international comparison of standard barometers, thermometers, and anemometers. This idea has since been abandoned, but M. Hooremann, Chef de Service of the Brussels Observatory, has visited Kew, with several standard instruments, in order to make a direct comparison between the Observatories of Brussels and Kew.

At the request of Miss Ormerod, F.M.S., experiments were made on the occasion of testing some thermometers at very low temperatures to determine the effect of great cold upon the vitality of certain grubs and insects selected by her for trial.

The Superintendent has, with the consent of the Committee, submitted a paper to the Royal Society on "The Results of an Inquiry into the Periodicity of Rainfall," which was printed in the "Proceedings," vol. xxx, p. 200.

He has also read a paper before the Meteorological Society "On the Rate at which Barometric Changes traverse the British Isles," published in the "Quarterly Journal," vol. vi, p. 136.

The Committee, having memorialised the Under Secretary of State for the Colonies with reference to the establishment of an Observatory for magnetical and meteorological purposes at Hong Kong, has been gratified by the receipt of an announcement to the effect that the Governor of Hong Kong has been authorised to propose a vote for the establishment of an Observatory in that colony.

Workshop.—The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and Planing Machine, procured by Grants from either the Government Grant Funds or the Donation Fund, for the use of the Kew Observatory, have been kept in thorough order,

and many of them are in constant, and others in occasional use at the Observatory, but the funds of the Committee do not at present allow of the employment of a mechanical assistant, although one is much needed.

Library.—During the year the Library has received, as presents, the publications of

14 English Scientific Societies and Institutions, and

47 Foreign and Colonial Scientific Societies and Institutions.

Ventilation Experiments. — The Sub-Committee of the Sanitary Institute of Great Britain is still engaged in experiments on the ventilating power of cowls of different form, for which purpose space has been placed at its disposal in the experimental house. In addition to this, the Institute has recently erected a wooden hut with an elevated wooden platform over it in the park, at a sufficient distance from the Observatory to avoid the eddies in the wind caused by it and the adjacent buildings.

Observatory and Grounds.—The buildings and grounds have been kept in repair throughout the year, and the rooms in the basement and some of the upper rooms have been painted and whitened by the Board of Works.

No action having been taken by the Commissioners with respect to the footpath across the park, its temporary repair has been carried on at the expense of the Committee.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, First Assistant.
J. Foster, Verification Department.
J. W. Hawkesworth, Tabulation of Meteorological Curves.
H. McLaughlin, Solar Computations and care of Accounts.
F. G. Figg, Magnetic Observer.
E. G. Constable, Solar Observations and care of Library.
T. Gunter
C. Taylor } Verification Department.
H. Clements
A. Dawson, Photography.
W. Boxall, Office duties.
J. Dawson, Messenger and Care-taker.

J. Hillier, having been appointed Assistant to the Curator of the Museums in the Royal Gardens, Kew, resigned in December last.

Visitors.—The Observatory has been honoured by the presence, amongst others, of:—

Professor Barrett.
Mr. Campbell.
Dr. C. Chistoni.
Rev. J. E. Cross.
Captain M. Hépites.
Mr. Hartnup.
Professor Libbey.
Professor Niven.
M. Perrotint.
Mr. Baden Pritchard.
Mr. Stone.
M. Steen.
Admiral Stopford.

Abstract. Kew Observatory Receipts and Payments Account from November 1, 1879, to November 3, 1880.

Dr. RECEIPTS.

	£ s. d.	£ s. d.
To Balance from 1878–79		512 5 7
Royal Society (Gassiot Trust)	£218 17 5	
" "	247 16 3	
		496 13 8
Meteorological Office	100 0 0	
" "	100 0 0	
" "	100 0 0	
" "	100 0 0	
		400 0 0
Meteorological Office, for Postages, &c.		35 12 9
Payment for Instruments on Commission		207 6 11
Sale of Waxed Paper		56 4 0
Verification Fees, Meteorological Office	86 13 0	
" " Observatories and Institutions	23 4 0	
" " Opticians, &c.	337 6 0	
Sale of Standard Thermometers	19 17 8	
Payments for Copying Registers	71 13 10	
		538 14 6
Mr. De La Rue for Sun-work		100 0 0
Solar Committee of Science and Art Department		0 18 0
Sale of Old Magnetometer		5 0 0
" Photographic Residues		11 14 11
		£2364 10 4

PAYMENTS. *Cr.*

	£ s. d.	£ s. d.
By Salaries	£1024 14 0	
Extra Payments	67 3 1	
		1091 17 1
Rent of Enclosure	11 11 0	
Fuel and Gas	74 6 10	
Furniture and Fittings	19 8 9	
Chandlery, &c.	7 7 9	
Painting and Repairs	23 15 0	
Maintenance of Path and Fence	13 2 3	
		149 11 7
Printing and Stationery	52 5 2	
Postages	15 1 5	
Library	11 12 2	
Messenger and Housekeeper	63 12 0	
Advertisements	1 1 3	
Porterage and Contingencies	15 5 7	
		158 17 7
Instruments purchased on Commission		209 6 6
Postages and Payments on behalf of Meteorological Office		8 14 6
Purchase of Waxed Paper, Packing ditto, &c.		81 0 9
" " Chemicals and Materials	26 12 5	
" " Tubes for Standard Thermometers	5 5 0	
Verification Department Expenses (Ice, Carbonic-Acid Gas, &c.)	10 1 0	
Repair of Instruments, and Purchase of New	50 7 6	
Carpenter's Work and Sundries	1 19 7	
		94 5 6
Sun-work expenses		100 0 0
Preparing Reference Catalogue of Schwabe's Sun-spot Drawings		89 19 5
Special Experimental Fund		4 17 6
Balance—Bank of England	294 9 7	
London and County Bank	75 0 0	
Cash in hand	6 10 4	
		375 19 11
		£2364 10 4

November 12, 1880. Examined and compared with the Vouchers, and found correct. (Signed) WM. GRYLLS ADAMS, *Auditor*.

ASSETS.

	£ s. d.
By Balance as per Statement	375 19 11
Meteorological Office, Allowances and Sundries	33 14 1
Commissions	51 5 0
Waxed Paper in stock	54 2 6
Blank Forms	2 10 0
Verification Fees due	82 5 11
Standard Thermometers sold	21 13 6
" " in stock	88 13 6
Sundries	4 2 1
	£714 6 6

LIABILITIES.

	£ s. d.
To Payments due for Extra Observations	9 2 6
Gas, Fuel, and House Account	11 10 9
Apparatus, Chemicals, &c.	18 13 3
Commissions	36 10 0
Balance	638 10 0
	£714 6 6

(Signed) G. M. WHIPPLE

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N., Long.* $0^h\ 1^m\ 15^s.1$ *W., for the year October* 1879 *to September* 1880.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets N D and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00109$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have all been made and reduced by Mr. F. G. Figg.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.
1879.	d. h. m.	foot.		° ′ ″	
October........	27 12 32 P.M.	1·0	55·8	15 34 5	9·12936
		1·3	....	7 1 14	
	2 30 „	1·0	57·0	15 33 28	
		1·3	....	7 0 56	
November......	25 12 28 P.M.	1·0	39·5	15 35 14	9·12899
		1·3	....	7 1 40	
	2 36 „	1·0	41·4	15 35 27	
		1·3	....	7 1 51	
December......	22 12 22 P.M.	1·0	41·0	15 35 37	9·12904
		1·3	....	7 1 58	
	2 23 „	1·0	43·0	15 34 41	
		1·3	....	7 1 30	
1880.					
January........	27 12 25 P.M.	1·0	23·6	15 37 10	9·12890
		1·3	....	7 2 55	
	2 22 „	1·0	23·5	15 37 18	
		1·3	....	7 2 46	
February	24 12 29 P.M.	1·0	42·2	15 35 29	9·12902
		1·3	....	7 1 53	
	2 36 „	1·0	42·2	15 34 42	
		1·3	....	7 1 30	
March	25 12 26 P.M.	1·0	54·8	15 33 6	9·12898
		1·3	....	7 0 52	
	2 30 „	1·0	60·7	15 32 6	
		1·3	....	7 0 27	
April..........	27 12 30 P.M.	1·0	52·1	15 32 51	9·12853
		1·3	....	7 0 41	
	2 37 „	1·0	54·4	15 31 57	
		1·3	....	7 0 18	
May	24 12 34 P.M.	1·0	65·6	15 29 51	9·12836
		1·3	....	6 59 27	
	2 34 „	1·0	65·0	15 30 17	
		1·3	....	6 59 42	
June	29 12 34 P.M.	1·0	76·3	15 29 15	9·12861
		1·3	....	6 59 6	
	2 24 „	1·0	77·7	15 28 34	
		1·3	....	6 58 48	
July	26 12 32 P.M.	1·0	71·8	15 29 25	9·12848
		1·3	....	6 59 23	
	2 40 „	1·0	67·9	15 29 42	
		1·3	....	6 59 25	
August	23 12 39 P.M.	1·0	64·6	15 32 45	9·12927
		1·3	....	7 0 38	
	3 7 „	1·0	68·1	15 31 15	
		1·3	....	7 0 3	
September......	28 12 43 P.M.	1·0	63·3	15 32 3	9·12900
		1·3	....	7 0 25	
	2 37 „	1·0	67·2	15 31 18	
		1·3	....	6 59 49	

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.*	Log mX. Mean.	Value of m.†
1879.	d. h. m.	°	secs.		
October..........	27 11 58 A.M.	55·0	4·6376		
	3 3 P.M.	58·2	4·6380	0·31143	0·52528
November........	25 11 45 A.M.	36·9	4·6301		
	3 8 P.M.	41·4	4·6335	0·31150	0·52510
December........	22 11 39 A.M.	39·5	4·6316		
	2 53 P.M.	44·3	4·6292	0·31192	0·52539
1880.					
January..........	27 11 47 A.M.	21·5	4·6285		
	2 57 P.M.	23·0	4·6298	0·31103	0·52476
February	24 11 50 A.M.	40·7	4·6359		
	3 8 P.M.	41·9	4·6350	0·31093	0·52477
March...........	25 11 45 A.M.	52·8	4·6393		
	3 3 P.M.	62·4	4·6403	0·31111	0·52486
April............	27 11 52 A.M.	50·6	4·6405		
	3 14 P.M.	54·5	4·6405	0·31067	0·52432
May.............	24 11 57 A.M.	63·5	4·6423		
	3 10 P.M.	65·7	4·6418	0·31117	0·52452
June	29 11 46 A.M.	75·8	4·6504		
	3 34 P.M.	79·1	4·6472	0·31067	0·52437
July.............	26 11 53 A.M.	69·3	4·6450		
	3 14 P.M.	70·4	4·6432	0·31102	0·52451
August	23 12 5 P.M.	64·2	4·6483		
	3 35 P.M.	68·7	4·6467	0·31018	0·52447
September........	28 12 10 P.M.	62·1	4·6476		
	3 16 P.M.	69·1	4·6493	0·30996	0·52418

* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

† m = magnetic moment of vibrating magnet.

Dip Observations.

Month.	G. M. T.	Needle.	Dip.
	d. h. m.	No.	North.
1879. Oct.	28 3 7 P.M.	1	67° 42′·87
	3 3 ,,	2	42·75
	30 2 59 ,,	1	42·81
	2 59 ,,	2	42·69
	Mean..	....	67 42·78
Nov.	26 3 10 P.M.	1	67 42·68
	3 6 ,,	2	41·68
	27 3 4 ,,	1	42·18
	3 6 ,,	2	41·62
	Mean..	....	67 42·04
Dec.	23 3 4 P.M.	1	67 42·62
	3 4 ,,	2	42·12
	24 3 23 ,,	1	42·56
	3 24 ,,	2	41·75
	Mean..	....	67 42·26
1880. Jan.	28 3 5 P.M.	1	67 41·87
	3 3 ,,	2	41·37
	29 3 14 ,,	1	41·00
	3 11 ,,	2	41·87
	Mean..	....	67 41·53
Feb.	17 3 16 P.M.	1	67 42·37
	3 20 ,,	2	42·68
	19 3 10 ,,	1	42·18
	3 13 ,,	2	41·37
	23 3 16 ,,	1	42·00
	3 15 ,,	2	41·12
	25 3 1 ,,	1	41·12
	2 58 ,,	2	41·50
	27 3 18 ,,	1	42·00
	3 26 ,,	2	41·00
	Mean..	...	67 41·73
Mar.	24 2 55 P.M.	1	67 42·06
	2 54 ,,	2	41·62
	30 2 58 ,,	1	42·06
	2 58 ,,	2	41·31
	31 3 8 ,,	1	42·12
	3 7 ,,	2	41·18
	Mean..	....	67 41·72

Month.	G. M. T.	Needle.	Dip.
	d. h. m.	No.	North.
1880. Apl.	26 2 56 P.M.	1	67° 42′·56
	2 55 ,,	2	42·12
	28 3 12 ,,	1	40·43
	3 13 ,,	2	40·75
	29 3 0 ,,	1	41·93
	2 59 ,,	2	41·25
	Mean..	....	67 41·51
May	25 3 14 P.M.	1	67 41·43
	3 13 ,,	2	41·37
	27 3 11 ,,	1	42·00
	3 10 ,,	2	41·43
	Mean..	....	67 41·56
June	24 3 14 P.M.	1	67 41·68
	3 17 ,,	2	40·75
	30 4 16 ,,	1	41·81
	4 21 ,,	2	40·93
	Mean..	....	67 41·29
July	28 3 3 P.M.	1	67 42·37
	3 3 ,,	2	41·62
	29 3 11 ,,	1	41·68
	3 14 ,,	2	40·81
	30 3 4 ,,	1	41·87
	3 5 ,,	2	41·06
	Mean..	....	67 41·57
Aug.	27 3 40 P.M.	1	67 43·43
	3 34 ,,	2	43·37
	31 3 10 ,,	1	42·68
	3 13 ,,	2	41·93
	Mean..	....	67 42·85
Sept.	29 2 58 P.M.	1	67 44·81
	3 0 ,,	2	44·25
	30 3 17 ,,	1	43·62
	3 14 ,,	2	42·75
	Mean..	....	67 43·86

Month.	Declination.	Magnetic Intensity.								
		English Units.			Metric Units.			C. G. S. Units.		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1879. October	19° 2′ 20″	3·8997	9·5146	10·2828	1·7981	4·3870	4·7412	0·1798	0·4387	0·4741
November	18 58 10	3·9017	9·5135	10·2825	1·7990	4·3865	4·7411	0·1799	0·4386	0·4741
December	19 0 47	3·9034	9·5194	10·2887	1·7998	4·3892	4·7439	0·1800	0·4389	0·4744
1880. January	18 58 24	3·9000	9·5054	10·2742	1·7982	4·3828	4·7373	0·1798	0·4383	0·4737
February......	18 59 47	3·8990	9·5045	10·2733	1·7978	4·3824	4·7369	0·1798	0·4382	0·4737
March	18 59 26	3·9000	9·5069	10·2757	1·7982	4·3835	4·7379	0·1798	0·4383	0·4738
April	18 58 29	3·9000	9·5054	10·2745	1·7982	4·3828	4·7374	0·1798	0·4383	0·4737
May..........	19 0 2	3·9030	9·5131	10·2825	1·7996	4·3863	4·7411	0·1800	0·4386	0·4741
June	19 1 7	3·8997	9·5028	10·2719	1·7981	4·3816	4·7362	0·1798	0·4382	0·4736
July..........	18 58 26	3·9018	9·5102	10·2795	1·7991	4·3850	4·7397	0·1799	0·4385	0·4740
August	18 56 24	3·8945	9·5023	10·2695	1·7957	4·3814	4·7351	0·1796	0·4381	0·4735
September	19 0 41	3·8948	9·5111	10·2776	1·7958	4·3854	4·7388	0·1796	0·4385	0·4739

APPENDIX II.

Meteorological Observations.—Table I.

Kew Observatory.

Longitude $0^h\ 1^m\ 15^s{\cdot}1$ W. Latitude 51° 28′ 6″ N. Height above sea-level = 34 feet.

Mean Monthly results from the continuous Records for the Twelve Months ending September 30th, 1880.

Months.	Thermometer.* Means.	Thermometer.* Extreme maximum. Date.	Thermometer.* Extreme maximum. Ther.	Thermometer.* Extreme minimum. Date.	Thermometer.* Extreme minimum. Ther.	Barometer.† Means.	Barometer.† Extreme maximum. Date.	Barometer.† Extreme maximum. Bar.	Barometer.† Extreme minimum. Date.	Barometer.† Extreme minimum. Bar.	Pressure. Means. Vapour-tension.	Pressure. Means. Dry air.
	°	d. h.	°	d. h.	°	inches.	d. h.	inches.	d. h.	inches.	inch.	inches.
1879. October....	49·4	4 3 P.M.	64·4	26 4 A.M.	32·9	30·132	12 10 A.M.	30·569	20 6 A.M.	29·373	·305	29·827
November..	38·8	18 2 „	54·9	16 7 „	21·9	30·221	{7 midt. 8 1 A.M.}	30·642	12 1 „	29·748	·201	30·020
December..	32·7	31 8 „	53·9	7 9 „	13·5‡	30·328	23 3 „	30·822	4 {8 P.M. 11 „}	29·520	·167	30·161
1880. January....	33·2	1 1 „	55·0	{27 10 P.M. 28 0.20 A.M.}	18·9‡	§30·387	7 10 „	30·687	1 1 A.M.	29·798	·165	30·222
February...	41·9	20 3 „	53·2	2 7 „	25·0	29·813	25 9 „	30·484	17 7 „	28·826	·232	29·581
March.....	44·5	25 3 „	60·6	29 7 „	27·7	30·118	8 10 „	30·513	3 {2 „ 3 „ 4 „}	29·256	·229	29·884
April......	47·3	19 3 „	64·6	8 5 „	34·7	‖29·879	30 7 „	30·446	6 5 „	29·273	·246	29·633
May	51·9	26 2 „	81·0‡	2 5 „	32·2	30·089	29 8 „	30·487	27 6 „	29·686	·267	29·822
June	57·7	29 5 „	76·0	5 4 „	37·4	29·914	27 {10 P.M. 11 „}	30·239	7 1 P.M.	29·594	·363	29·551
July.......	61·6	15 6 „	76·9	31 5 „	48·6	29·901	5 8 A.M.	30·195	26 5 „	29·450	·420	29·481
August	62·6	28 3 „	77·6‡	3 4 „	49·7	29·998	10 {8 „ 10 „}	30·314	7 9 „	29·204	·436	29·562
September..	59·5	4 2 „	84·0	20 4 „	44·0	29·985	29 10 „	30·511	15 4 A.M.	29·147	·407	29·578
Means.........	48·4	....	..	....	..	30·063	....	..	....	..	·286	29·777

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometer-bulbs are 10 feet above the ground.

† Readings reduced to sea-level.

‡ Approximate reading.

§ One of the daily means doubtful.

‖ Three of the daily means doubtful.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather †. Number of days on which were registered						Wind ‡. Number of days on which it blew							
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1879.		in.	in.															
October ..	7·5	0·700	0·220	24	14	..	..	..	1	16	2	9	4	..	..	6	6	4
November	6·9	0·765	0·250	21	10	4	..	..	4	14	12	4	1	..	1	3	5	4
December	7·9	0·775	0·300	30	9	4	1	1	2	23	3	7	3	1	5	7	3	2
1880.																		
January ..	7·4	0·440	0·210	16	6	2	..	..	3	16	7	3	5	3	3	4	4	2
February .	7·7	2·215	0·400	7	19	..	2	..	2	16	3	..	..	1	10	11	3	1
March ...	5·3	0·730	0·285	31	6	..	..	..	9	12	1	9	8	3	1	5	4	..
April	7·4	1·975	0·430	14	16	..	2	3	..	13	4	6	2	..	5	8	4	1
May	6·7	0·280	0·215	31	4	..	..	1	2	13	6	10	4	..	..	5	3	3
June	7·5	2·215	0·440	15	19	..	..	3	..	15	4	5	3	..	3	7	6	2
July	7·5	4·890	0·535	25	19	..	..	12	..	18	2	2	1	1	5	14	5	1
August ..	7·5	0·555	0·215	2	8	..	..	1	2	20	6	14	1	..	1	3	4	2
September	6·5	4·395	1·440	11	14	..	1	2	5	13	1	2	3	2	4	8	9	1
Totals..		19·935			144	10	6	23	30	189	51	71	35	11	38	81	56	23

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † Derived from observations made at 10 A.M., noon, 2, 4, and 10 P.M.

‡ As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.*		Maximum temperature in sun's rays.			Minimum temperature on the ground.			Horizontal movement of the Air.†		
	Total number of hours.	Number of hours Sun was above the horizon.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average daily Velocity.	Greatest Movement in a day.	Date.
1879.	h. m.	h. m.	deg.	deg.		deg.	deg.		miles.	miles.	
October	69 12	329 24	87·7	113·6	4	38·9	26·0	17	204	431	30
November	45 30	264 54	69·8	94·2	30	28·4	12·2	16	180	415	12
December	18 0	243 3	52·3	81·4	30	22·8	8·0	7	186	624	28
1880.											
January	46 30	257 33	54·0	83·1	31	25·6	11·1	28	159	459	1
February	67 24	287 43	77·3	99·5	27	32·2	22·0	12	297	581	9
March	141 54	367 45	92·0	108·9	14	33·3	22·1	24	330	749	2
April	127 24	415 34	104·7	123·2	24	36·0	27·5	26	311	500	29
May	193 30	482 50	117·9	132·0	26	36·9	24·0	1	264	491	16
June	153 24	494 29	119·9	138·7	26	45·1	27·3	5	220	405	7
July	191 0	496 30	128·5	138·8	22	50·7	42·4	5	223	398	28
August	138 0	448 38	120·0	138·2	6	52·4	42·2	3	221	363	12
September	140 30	376 42	116·6	131·5	6	45·2	36·2	20	167	360	18

* Registered by the Sunshine-recorder.

† As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Errata in Kew Report, 1878–9.

By error no corrections for height above sea-level were applied to the extreme barometer readings in the Table on page 462.

The following values must therefore be substituted for those printed under extreme maximum and extreme minimum respectively.

		Inches.		Inches.
1878.	October	30·352	......	29·026
„	November	30·487	......	29·205
„	December	30·387	......	29·159
1879.	January	30·457	......	29·349
„	February	30·182	......	28·834
„	March	30·633	......	29·566
„	April	30·337	......	28·934
„	May	30·528	......	29·543
„	June	30·165	......	29·407
„	July	30·177	......	29·303
„	August	30·334	......	29·465
„	September	30·497	......	29·297

Presents, November 18, 1880.

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an occasional visit to the Continent, either for duty or for relaxation. He delighted especially in the scenery of the dolomite mountains of the Italian Tyrol, spending among them many hours of quiet enjoyment, while their magnificent outlines were recorded with rare fidelity by the accomplished companion of his life.

Happy, then, in his domestic life, happy in the affectionate appreciation of numerous friends of varied ages and ranks, he was also happy in seeing his work (though for honours and rewards he cared less than most men) not unacknowledged by his contemporaries. In addition to the honours mentioned above, he received in 1865 the degree of LL.D. from the University of Dublin, and in 1876 that of D.C.L. from Oxford. In 1870 he was awarded a Royal Medal by this Society. He was a Knight of the Order of St. Maurice and St. Lazare of Italy and of the Order of Leopold of Belgium. He was also an honorary member of the Royal Society of Edinburgh, of the Mineralogical Society of France, and of Boston, U.S.A., a foreign member of the Mineralogical Society of St. Petersburg, of the Imperial Royal Academy of Sciences, Vienna, and of the Royal Society, Göttingen; and a corresponding member of the Academies of Berlin, Munich, Paris, St. Petersburg, and of Turin.

WILLIAM LASSELL, LL.D., died at Maidenhead, October 5, 1880, aged eighty-two years. He was born at Bolton, Lancashire, June 18, 1799. He acquired the rudiments of education at a day school in his native town, during which time his father died, and thence went for eighteen months to an academy at Rochdale.

In 1814 he entered a merchant's office at Liverpool, and there served a seven years' apprenticeship. He commenced business in Liverpool as a brewer about the year 1825, without, however, much taste or inclination for trade, and spent almost all his leisure time in his favourite pursuit of astronomy and the mechanics connected therewith.

Mr. Lassell possessed a great love and aptitude for mechanical invention, and for this reason "he belonged," to use the words of Sir John Herschel, "to that class of observers who have created their own instrumental means, who have felt their own wants, and supplied them in their own way." The qualities which enabled Mr. Lassell to do all this made him what he was. The work was the revelation of the man. He felt precisely where lay the difficulties and wants which met him in his work, because he was sensitive and sympathetic. He could deal successfully with these difficulties and supply these wants often in a masterly and original way, because he could think for himself cautiously yet boldly. He could work out his conceptions in new and difficult directions to a successful issue, because the constancy of his character showed itself here in concentration of thought and perseverance of

action. These qualities—sensitive sympathy, wise prudence, constancy—were those which pre-eminently characterised him as a man, and made him to those who knew him a friend of rare worth.

In the history of science Mr. Lassell's name will rank with those of Herschel and the late Lord Rosse in connexion with that essentially British instrument, the reflecting telescope, whether we consider the genius and perseverance displayed in the construction of these instruments or the important discoveries which have resulted from their use. About 1820 Mr. Lassell, then in his twenty-first year, began to construct reflecting telescopes for himself. It is perhaps to circumstances which he at the time considered unfavourable, that science is indebted for much that Mr. Lassell has accomplished. At that time he did not possess sufficient means to enable him to purchase expensive instruments, and besides "his business avocations were such as most men consider of an engrossing nature." The value to him in his subsequent work of the energy and power of resource which were in this way so strongly developed in his character at an early age, it is difficult rightly to appraise. His success with the first two instruments, which he attempted simultaneously (a Newtonian of seven inches diameter and a Gregorian of the same size), encouraged him to make a Newtonian of nine inches aperture. The instrument, which was erected in an observatory at his residence near Liverpool, happily named Starfield, may be said to form an epoch in the history of the reflecting telescope, in consequence of the successful way in which Mr. Lassell, on a plan of his own, secured to it the inestimable advantages of the equatorial movement. The several mirrors made for this instrument were of great excellence. The observatory note-books of the late Mr. Dawes, which are in the writer's possession, bear record to the delicate tests for figure to which these mirrors were put on the occasions of the frequent visits of Mr. Dawes to his friend's observatory. With this instrument Mr. Lassell diligently observed, and detected, without knowledge of its existence, the sixth star in the trapezium of the nebula of Orion. This instrument is fully described in the twelfth volume of the "Memoirs of the Royal Astronomical Society."

About the year 1844 Mr. Lassell conceived the bold idea of constructing a reflector of two feet aperture and twenty feet local length, to be mounted equatorially on the same principle. He spared neither pains nor cost to make this instrument as perfect as possible, both optically and from the mechanical side. As a preliminary step, he visited the late Earl of Rosse at Birr Castle, and commenced the specula for the new instrument upon a machine similar in construction to that employed by that nobleman. After some months' work, he was not satisfied with this apparatus, and was led in consequence to contrive a machine for imitating as closely as possible those motions of the hand by which he had been accustomed to produce perfect surfaces on

smaller specula. "The essential difference of these constructions," to use the words of Sir George Airy, "as regards the movements of the grinder is this: that in Lord Rosse's apparatus every stroke is very nearly straight, while in Mr. Lassell's apparatus there is no resemblance to a straight movement at any part of the stroke." This is not the place to describe the many new contrivances in the mode of support of the mirror, in the equatorial mounting, in the polishing machine, and in the arrangements of the dome under which it was erected, which enabled Mr. Lassell to bring his telescope to a high degree of perfection. In this connexion it should be noticed, to use Sir John Herschel's words, "that in Mr. Nasmyth he was fortunate to find a mechanist capable of executing in the highest perfection all his conceptions, and prepared by his own love of astronomy and practical acquaintance with astronomical observations, and with the construction of specula, to give them their full effect."

With this fine instrument he discovered the satellite of Neptune. This minute body was seen on October 10th, 1846, but it was not until the next year that it could be satisfactorily followed, and its existence fully confirmed. The superiority of the telescope, and the vigilance and skill of the observer were further shown by the discovery in 1848, simultaneously with Professor Bond in America, of an eighth satellite of Saturn, of extreme minuteness, which was named Hyperion. In 1851, after long and careful search, he discovered two additional satellites of the planet Uranus (Umbriel and Ariel), interior to the two discovered by Sir William Herschel in 1787.

In the autumn of 1852, he took this twenty-foot telescope to Malta, and observed with it there through the winter. A very careful and detailed drawing of the nebula of Orion, and drawings of several planetary nebulæ made at Malta, will be found in vol. xxiii of the "Memoirs of the Royal Astronomical Society." With respect to the planets, his discoveries, to use his own words, "were rather negative than otherwise," for he was satisfied, that without great increase of optical power, no other satellite of Neptune could be detected. With regard to Uranus, he says, "I am fully persuaded that either he has no other satellites than the four, or if he has, they remain yet to be discovered."

Mr. Lassell's energy and zeal in the cause of science did not permit him to remain content with this magnificent telescope. His last work was a much larger telescope, four feet in aperture, and thirty-seven feet focus, mounted equatorially, which was erected at Malta, in 1861. The work done with it, with Mr. Marth's assistance, during the next four years, is fully described, as well as the instrument itself, in vol. xxxvi of the "Memoirs of the Royal Astronomical Society." This work consists of numerous observations of nebulæ and planets, and a catalogue of the places of 600 new nebulæ

discovered at Malta. It is not possible to suppress a feeling of regret that this grand instrument no longer exists.

Mr. Lassell was very successful in the great brilliancy and permanence of polish of his mirrors. Within the last few years the writer has been shown specula by Mr. Lassell, which had been polished for more than twenty years, and which appeared as bright as if but just removed from the polishing machine. His earlier metal differed from that employed by Lord Rosse, in that it contained a small quantity of arsenic in addition to copper and tin. In the case of his four-foot mirrors, the arsenic was omitted. He seems to have considered that the perfection of the metal depended upon the accurate relative proportion of the copper to the tin, and that, in consequence of the uncertainty of the state of purity of the metals, this proportion would be obtained with the necessary accuracy only by a series of testings, while the metal was in the pot.

After his return from Malta, Mr. Lassell purchased a residence near Maidenhead, and erected there in an observatory, his equatorial telescope of two-foot aperture. Mr. Lassell's experience in re-polishing his four-foot mirror, suggested to him some alterations in his polishing machine. After his return, he was able to carry out experiments in connexion with the suggested alterations in a workshop erected at Maidenhead, and succeeded in constructing an improved form of polishing machine, which is described in the "Philosophical Transactions" for 1874. The numerous papers by Mr. Lassell, to be found in the "Monthly Notices," and the "Memoirs" of the Royal Astronomical Society, bear abundant record to his industry and skill, and make us feel that in Mr. Lassell's death we have to deplore the loss of one who contributed largely to the advancement of the science of his age.

Mr. Lassell was elected a Fellow of the Royal Astronomical Society in 1839, he received the Gold Medal of that Society in 1849, and in 1870 was elected its President, which office he held for two years. He became a Fellow of the Royal Society in 1849, and received one of the Royal Medals in 1858. Among other honours conferred upon him, may be mentioned an honorary degree from the University of Cambridge, and the honorary Fellowship of the Royal Society of Edinburgh, and of that of Upsala.

Dr. William Sharpey was born at Arbroath, in Forfarshire, Scotland, on the 1st of April, 1802. His father was an Englishman and belonged to Folkestone, in Kent, till the year 1794, when he migrated to Arbroath, and there married Mary Balfour, a native of that place; but, he dying shortly before the birth of his son William, Mrs. Sharpey was afterwards married to Dr. William Arrott, a medical practitioner of Arbroath, in whose family the subject of this notice was brought up.

William Sharpey's education was carried on up to the age of fifteen at the public school of Arbroath. In November, 1817, he entered the University of Edinburgh as a student in the Faculty of Arts, attending the Greek and Natural Philosophy classes.

In 1818 he commenced his medical studies, in the University and the extra-academical school of Edinburgh. In the latter school his teacher in anatomy was Dr. John Barclay, well known for his energy as a writer and lecturer and his ingenuity as an observer in human and comparative anatomy. In chemistry his chief instructor was Dr. John Murray, of whose scientific accuracy and judgment Dr. Sharpey always spoke in the warmest terms. In 1821, at the age of nineteen, he obtained the diploma of the Edinburgh College of Surgeons. He then passed some months of that year in the study of anatomy at Brookes' School in London, and in the autumn proceeded to Paris and remained there for nearly a year, in attendance upon the medical and surgical wards of the hospitals, more especially in the surgical wards of the Hotel Dieu, under the instruction of the celebrated Dupuytren. In Paris he had the companionship of his friend Dr. Robert Willis, and he first made the acquaintance of Professor Syme, with both of whom he was ever after on the most intimate and friendly terms.

In August, 1823, he took the degree of Doctor of Medicine in the University of Edinburgh, his printed inaugural dissertation bearing the title "De Ventriculi Carcinomate;" and in the latter part of the year he went again to Paris to complete his studies in medicine and surgery at the hospitals, and in natural history at the Garden of Plants. After his return from France in the summer of 1824, his plans seem to have remained for some time undecided until near the end of 1826, when he finally resolved not to embark in medical practice, but to devote himself to anatomical and physiological pursuits, for which he had long had a predilection, and to the study of which he had already given a considerable share of attention.

With this view he resolved upon completing his continental travels, and obtaining the advantages of study in the Italian and German schools, as he had already done very fully in the French; and accordingly, in the autumn of 1827, he proceeded by way of Paris and Geneva to Switzerland, in which he made a three months' pedestrian tour. He then travelled in the north of Italy, visiting Milan, Pavia (where he spent some time in company with Panizza), Genoa, and Florence. The winter was passed in Rome, Naples, and other places of interest in central Italy; and in the spring of 1828 he turned his steps northward, taking the way of Bologna, Padua, and Venice, to Verona and Innsbruck. The summer was spent in Austria and North Germany, and he finally reached Berlin in August, where it was his main object to devote himself to the study of anatomy. In

doing this he had the inestimable advantage of the assistance and friendship of Professor Rudolphi; and for nine months he gave the whole of his time with the closest application to the minute and full dissection of the human body,—the only way, as he himself expressed it, in which any one could obtain the knowledge necessary to a competent teacher of the subject.

As already mentioned, Dr. Sharpey made considerable parts of his foreign travels on foot, with his knapsack on his back, picking up acquaintance with fellow travellers as he went, mixing with the natives of the several places he visited, and storing up in his wonderfully tenacious memory that fund of observation, anecdote, and incident, which always surprised and delighted those who afterwards heard him narrate his travels.

On his return from the Continent, in the autumn of 1829, he established himself in Edinburgh, and engaged in microscopic observation and scientific anatomical research; and, in 1830, as a necessary preliminary to his being qualified as a teacher, he obtained the Fellowship of the College of Surgeons, and presented a probationary essay "On the Pathology and Treatment of False Joints" (after fracture); a subject which was no doubt suggested by his intimacy with Mr. Syme, to whom the printed essay is inscribed. In the summer of 1831, Dr. Sharpey again spent three months in Berlin, on this occasion being chiefly employed in collecting anatomical preparations and other materials for the illustration of the course of instruction in anatomy which he had in view to deliver in the following winter. This long-cherished object he carried into effect by giving, during the session of 1831–32, a course of systematic lectures on anatomy in the extra-academical school of Edinburgh in association with Dr. Allen Thomson, who taught physiology. This association subsisted during the four following years of Dr. Sharpey's stay in Edinburgh. At this time a keen competition existed among the four teachers who, in addition to the Professor within the University, divided among them the students who applied for instruction; and as Dr. Sharpey's class increased during the period mentioned from twenty-two to eighty-eight, we may regard his success as complete in point of number, while his reputation as a teacher and man of science had advanced in a still greater degree, so that he had now come to be generally known both in the seat of his labours and at a distance as one of the most judicious, learned, and accurate investigators and teachers of his favourite science.

From 1829 to 1836, Dr. Sharpey was also actively engaged in scientific investigations; among which the earliest and perhaps the most novel and important were those on ciliary motion, described in a paper published in 1830 ("On a peculiar Motion excited in Fluids by the surfaces of Certain Animals," "Edin. Med. and Surg.

Journal," vol. civ, 1830). By the observations which were described in this paper, many of which were entirely new, Dr. Sharpey appears to have been the first to point out distinctly the general distribution among animals, and the essential nature and uses of the phenomena of ciliary motion; and although it is true that he afterwards found he had been anticipated in one of the most important of his observations, and that at the time of his first publication, from the want of a sufficiently powerful microscope, he was unsuccessful in detecting the presence of cilia in the Batrachia, his later observations led to that result, and his observations on a number of animals greatly amplified and confirmed the general conclusions which followed from the important discovery by Purkinje and Valentin in 1834, of the existence of cilia in vertebrate animals.

In 1835, Dr. Sharpey published in "The Edinburgh New Philosophical Journal," a translation of the preliminary memoir in which the discovery of Purkinje and Valentin was announced, and at the same time gave an account of additional observations on the subject made by himself; and he soon afterwards embodied the whole of the information on Cilia and ciliary motion in a systematic form in his article "Cilia," published in the "Cyclopædia of Anatomy and Physiology," in 1836, but which he had been engaged in preparing for several years previously.

Dr. Sharpey also contributed the article "Echinodermata," which appeared in the same publication in 1837, and which, like that of "Cilia," contained a large amount of original matter, and added greatly to his scientific reputation.

In 1833 he published an abstract of Ehrenberg's discoveries on the Infusoria in "The Edinburgh New Philosophical Journal." In 1834 he took an active part in the proceedings of the Meeting of the British Association at Edinburgh, and communicated a paper founded on his own observations on the peculiar convoluted disposition of the blood-vessels in the common porpoise.

In 1834, Dr. Sharpey was elected a Fellow of the Royal Society of Edinburgh.

We now come to the period of Dr. Sharpey's career when he was about to be called to a wider sphere of exertion in the metropolis. In the summer of 1836, upon the resignation by Dr. Jones Quain of the chair of Anatomy and Physiology in the then University of London, a desire was felt by the leading professors and authorities of that institution to give greater prominence than had previously been done in the London schools of medicine to the subjects of physiology and physiological anatomy, and, after due inquiry, Dr. Sharpey was, in the course of July, selected as the fittest person to fill the chair and carry out the object in view. He was accordingly appointed to the chair designated as of Anatomy and Physiology, while Mr.

Richard Quain was named Professor of Anatomy; it being determined that Dr. Sharpey should in his course treat fully of physiology, or of the functions of the body along with minute and visceral anatomy, and that Professor Quain should occupy himself with the descriptive and practical departments of anatomy. There was thus established in London, for the first time, the full and systematic teaching of physiology, which had previously been only imperfectly treated as an appendage to the courses of anatomy in the London medical schools.

The great success of Dr. Sharpey as a teacher in his favourite departments of biology was from the first apparent in the large number of his pupils, the close attention and deep interest with which he was listened to, and the marked influence which he exercised on the minds of the students, and in all the affairs of the school with which he was now connected. With all the interests of University College, as it was named after the institution of the London University, he soon became identified, while at the same time he took an active part in the business of other scientific bodies of the metropolis.

Dr. Sharpey never wrote out his lectures, excepting an introductory one, and he delivered them all without any assistance from writing beyond very short jottings on small slips of paper. He made use of diagrams and pictorial illustrations as well as of anatomical preparations and physiological experiments, and he was one of the first to introduce the employment of the microscope for the practical illustration of his lectures. For this purpose he employed more than forty years ago a revolving table which still exists in the physiological laboratory of University College, and which enabled a number of persons in succession to observe through one microscope; the first attempt made in London to illustrate physiological lectures microscopically. And thus in later times, when the improvement of the apparatus and methods of experimenting had become greatly extended, he lent all his influence to the establishment of the practical teaching in physiology which has since been so fully carried out in the Jodrell Laboratory for practical physiological experiment and research.

Dr. Sharpey's course of instruction was continued much in the same form during the long period of thirty-eight years in which he held the chair; the same scrupulous care in the preparation of his lectures, and the same conscientious performance of his public duties which he had shown in the earlier and most vigorous periods of his life being maintained to the last; and when we consider the number of those who followed his instructions, varying, with the fluctuating numbers attending the school, between 100 and 350 in each year, the wide-spread influence of his teaching can easily be understood. Many

of his pupils now hold high and important positions in the medical profession and in other departments of science; and all of them are ready to acknowledge, and none more emphatically than the most distinguished among them, their debt of gratitude to their biological teacher, not alone for the exact and solid information which they derived from his instructions, but also for the scientific spirit and love of truth which he endeavoured to instil into their minds.

Dr. Sharpey was by no means a copious writer; and indeed, it may be said that from his extreme fastidiousness with regard to all that emanated from his pen, he was much too sparing of authorship. Accordingly, much of the original observation and thought on scientific subjects which cost him prolonged labour, and involved much research, was made known by him only through his lectures, or was published in a more or less fragmentary form in connexion with such systematic works as "Baly's Translation of Müller's Physiology," and "Quain's Anatomy." In the first of these works, it is well known that the excellent translator, who was a distinguished pupil of Dr. Sharpey's class, derived much assistance in his labours from his teacher; and several notable additions were made to the work by contributions from Dr. Sharpey's pen. Among these, one of the most important is that taking the modest form of a note, in which he gave an account of original observations made by himself on the structure of the uterine glands and membrana decidua, and brought forward the first rational explanation of the manner in which the human ovum comes in the commencement of pregnancy to be imbedded in the substance of the decidua. In 1843–46, Dr. Sharpey published, as joint editor with Professor Richard Quain, the fifth edition of Dr. Jones Quain's "Elements of Anatomy," which, from the amount of new matter introduced, and changes made by the editors, assumed almost the character of a new work. In this edition, the general anatomy was entirely re-written by Dr. Sharpey, and has ever since been looked upon as a standard work on the subject of which it treats, containing the record of a large number of original observations upon the minute structure and growth of bone, and on many other subjects. "The Anatomy of the Brain and Heart," "Of the Organs of Respiration and Voice," "Of Digestion and Reproduction," were also from his pen. With the three subsequent editions of this work, Dr. Sharpey remained connected as one of the editors till the time of his death.

In 1862, Dr. Sharpey delivered the "Address in Physiology," at the thirtieth annual meeting of the British Medical Association, held in London in that year; and, as President of the Biological Section of the British Association for the Advancement of Science, at the Dundee Meeting in 1867, he delivered an address, in which, as in the one previously mentioned, he ably reviewed the progress of physi-

ology, more especially as regards the application of exact methods of research to the investigation of physiological problems.

Dr. Sharpey was appointed one of the examiners in anatomy to the University of London, when that body obtained its charter to grant degrees in 1840, and continued to perform the duties of the office during the long period of twenty-three years. He was, at a later period, a Member of the Senate of the University. He was also during fifteen years one of the members appointed by the Crown on the General Council of Medical Education and Registration. He acted for some time as one of the Treasurers of the Council, and took a deep interest in the various subjects connected with medical education and polity, which claimed its attention. Dr. Sharpey was also a member of the Science Commission, which met under the presidency of the Duke of Devonshire, from 1870 to 1875, and, taking an active part in its proceedings, he aided greatly by his sagacity and knowledge the deliberations of that body. He was also one of the Trustees of the Hunterian Museum of the Royal College of Surgeons, and a member of many scientific societies of this and other countries. He received the honorary degree of LL.D. from the University of Edinburgh in 1859.

Dr. Sharpey's connexion with the Royal Society began by his election as Fellow, on the 9th of May, 1839. He became a Member of the Council in 1844–5, and was appointed one of the Secretaries in place of Mr. Bell in 1853. This office he held for nineteen years, or till 1872, when the failing condition of his eyesight, obliged him to resign. He was again chosen a Member of Council for the next two years.

All those who attended the Society, or took part in its proceedings, are well aware of the strong and steady interest which Dr. Sharpey took in all its affairs, and of the great amount of anxious care and judicious labour which he devoted to the promotion of its welfare. In the course of so long a membership and official connexion with the Society, there were necessarily many incidental pieces of business in which he was particularly engaged. It is sufficient to mention as among the more important of those which occupied his attention, the following, viz.:—1. The correct and speedy publication of the Society's Proceedings and Transactions which fell under his superintendence. 2. The introduction, in 1848, of the mode of electing Fellows, by which the Council is made primarily responsible for the selection of the persons to be elected by the Society. 3. The removal of the Society with its library and other property from Somerset House to Burlington House, in 1857, and the subsequent transference to the new apartments in that locality. 4. The compilation and publication of the Society's Catalogue of Scientific Papers, in which his extensive and accurate knowledge of scientific literature enabled him

to render most important assistance. It may be further mentioned, that in 1870, along with other members of the Society, he was active in the introduction of a new plan for the election of Councillors, by which effect was given to the opinion of the whole Council, instead of the members being nominated by the President as previously.

Up to the age of sixty-eight or seventy, Dr. Sharpey retained most of the vigour of his earlier years; but about the year 1871, and still more decidedly in the following year, while some other signs of advancing age showed themselves in the partial failure of his digestive and locomotive powers, the rapid increase of a cataract, which affected both eyes, together with some dulness of hearing, began to interfere seriously with the efficient and easy performance of his official duties, and to take much from his pleasure in society; and very soon these affections led to his retirement, first, as already stated, from the Secretaryship of the Royal Society, in 1872, and second, from his Professorship in University College, in 1874. These infirmities were partially remedied by the operation of extraction of the lens of the left eye, in May, 1873, and of that of the right eye in October, 1876; but the recovery was not sufficient to give him more than a very limited use of his sight.

About the same time, Dr. Sharpey became subject to occasional attacks of bronchitis, from exposure to cold. One of these, occurring in 1878, was of great severity, and in the present year a renewed attack of the same malady with which he was seized at the end of March proved fatal on the 11th April, or ten days after he had completed his seventy-eighth year. He was buried in the Abbey graveyard of Arbroath, his native town, on the 17th of April, and on the day of the departure from London, his remains were accompanied by a large number of his friends and former pupils to the Euston Station of the North-Western Railway.

In 1869 the friends and former pupils of Dr. Sharpey, being desirous to carry out a design which had been for some time in contemplation of showing their regard for him, and establishing a permanent memorial of his services to University College and to science, raised by subscription a sum of money for endowing a Sharpey Memorial Scholarship in connexion with University College, and for presenting to the college a portrait in oil, and a marble bust. The proceedings in connexion with the presentation of this memorial were naturally a source of much gratification to himself and to his friends.

In 1872 he made over his large and well-chosen biological library to University College, and at his death he bequeathed from the small property which he left a sum of £800 to increase the endowment of the Sharpey Scholarship in physiology.

Upon his retirement from his Professorship in 1874, Mr. Gladstone's Government accorded Dr. Sharpey an annual pension of £150 on

account of his eminent services as a public teacher and man of science.

From what has now been said it will be seen that Dr. Sharpey did not enter upon any active sphere of exertion, either as an investigator or as a teacher, till he had attained his twenty-eighth year; but with characteristic caution he was, during a number of years, preparing himself with the greatest diligence and care, by literary and scientific study, as well as by continental travel, for the duties of his after life.

As a scientific investigator he was characterised by scrupulous care and accuracy in all his observations, and by an extensive and intimate acquaintance with what was previously known on the subjects. Thus it happens that though, as already remarked, he cannot be regarded as a copious observer or extensive discoverer of new facts, yet all the observations he has recorded may be ranked as important contributions to science at the time when they were made, and the greater number of them have retained their value to the present day, notwithstanding that the subjects to which they belong may, from the advance of knowledge, have considerably changed their aspect.

As a systematic author there is everywhere apparent in his writings the same scrupulous accuracy and full knowledge of his subject, combined with a simplicity and clearness of statement, an appropriate choice of language, and a critical acumen, which have given them a high and lasting value. We have, it is true, to regret the fastidiousness which deterred him from more copious publication, but we may console ourselves with the reflection that all he did publish bears the stamp of excellence, and that in abstaining from more extended literary productions he was ever spending his time and energies in the instruction of his pupils and the advancement of the business of the scientific institutions with which he was connected.

Dr. Sharpey's usefulness and influence were probably more conspicuous in his labours as a public teacher than in any other capacity. During the forty-three years in which he was constantly occupied in giving lectures on Anatomy and Physiology, he devoted himself with ardour and perseverance to perfecting the information which he had to communicate to his pupils, and to extending and improving the means of illustrating his lectures, so that he was uniformly listened to with the closest attention and regarded as the highest authority on the subjects which he taught. Thus too, it happened that he was very frequently consulted by former pupils as well as by others with regard to the preparation and publication of memoirs or more extensive works which they had in contemplation, and it is easy to understand the advantages which accrued to those who appreciated and followed his advice, or the opposite effect which sometimes occurred from its being disregarded.

But the effect of Dr. Sharpey's teaching upon a large number of pupils did not proceed alone from the superiority of the information conveyed, or the implicit reliance which his pupils placed in the fulness, accuracy, and truthfulness of the statements of their teacher, but it was also due to, and greatly enhanced by, the feeling of friendly attachment, and even of filial affection amounting to reverence, which was inspired in the minds of the pupils by his uniform kindness, justice, and candour.

In the other public offices held by Dr. Sharpey during the greater part of the time of his residence in London, the superior qualities of his mind had equal scope in conducing to the efficiency and usefulness of his services. As an examiner in the University of London and afterwards as a member of the Senate, as Secretary of the Royal Society, as Member of the General Medical Council, as one of the Science Commissioners and a trustee of the Hunterian Museum, his extensive knowledge, unbiassed judgment, and strict impartiality, while they gave weight to his opinions and suggestions, aided largely in the promotion of measures favourable to the interests of science and the public good.

Of the more private features of Dr. Sharpey's life and character it is difficult for those who have been most intimate with him to express their estimate in sufficiently moderate terms. While he was universally admired for the extent and accuracy of his acquirements and respected for the soundness of his judgment, he was not less esteemed and beloved for the gentleness of his disposition, the kindness of his heart, and the geniality of his nature. His powers of memory, naturally good, were carefully cultivated by the systematic turn of his mind and strengthened by exercise. His friends remember with delight the readiness with which, in the course of conversation, he could call up a desiderated quotation, or supply a fact on some doubtful point in history, philosophy, or science, or tell humorously some anecdote which was equally apposite and amusing. He had not a single enemy, and he numbered among his friends all those who ever had the advantage of being in his society.

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GOVERNMENT FUND OF £4000

FOR THE PROMOTION OF SCIENTIFIC RESEARCH.

A Meeting of the Government-Fund Committee will be held in February, 1881. It is requested that applications to be considered at that Meeting be forwarded to the Secretaries of the Royal Society, Burlington House, before the 31st of December, 1880.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

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December 9, 1880.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Bishop of Limerick and Professor Asa Gray (Foreign Member) were admitted into the Society.

The Chairman announced that the President had appointed as Vice-Presidents:—

The Treasurer.
Mr. W. H. Barlow.
Dr. Hirst.
Sir James Paget.
General Strachey.

The following Papers were read:—

I. "On a Simplified Form of the Torsion-Gravimeters of Broun and Babinet." By Major J. HERSCHEL, R.E., F.R.S., Deputy Superintendent, Great Trigonometrical Survey of India. Received October 31, 1880.

The present communication anticipates one of greater length and extent, which I hope to be enabled to offer for publication in the "Proceedings" of the Society, the subject of which is the gravimeters mentioned in the above title. In case the account which I there give of these instruments should be delayed, it seems advisable to hasten, if possible, the time when a serviceable gravimeter shall be available to geodesists, by pointing out a method by which the same principle may be embodied in a simpler instrumental shape. It is not necessary for this purpose that a prior knowledge of the existing designs should be presumed. I hope to be able to show all that is essential without drawing upon any source but a moderate knowledge of physics.

Let us imagine a weight, P, suspended by two* parallel lines, flexible, but inelastic, and offering no resistance on their own account to torsion. Let their length be R, and their distance asunder $2r$. Also let θ be the angle through which P is turned by some external force. Then it may be shown that the force which P exerts in a

* There is no necessary restriction to *two* suspenders.

horizontal direction, tending to cause it to return to its position of rest, may be expressed by

$$P \cdot \frac{r}{R} \sin \theta \div \sqrt{1 - \frac{2r^2}{R^2} \text{ versin } \theta}$$

exerted at a distance r from the axis of motion. When $\frac{r}{R}$ is small, this may be represented by the simple function $P\frac{r}{R}\sin\theta$, which varies directly as the sine of the angle of detortion, being 0 at zero and 180°, and a maximum at 90°.

This is the ordinary law of torsion of a bifilar balance, in which the suspending lines are regarded as non-resisting.

Now suppose the weight P held fast at its position of rest while the upper ends of the suspending lines, no longer impotent, but endued with elasticity and a consequent power of resistance to torsion, are turned, severally, about their own individual axes, through an angle η. The force which will be thereby developed, in what we may now call the wires, will be a true "torsion" force. And, if I am not mistaken, it will vary directly as the angle of torsion, and inversely as the length of the wire, but not as the tension nor as the distance of the wires from each other.

Let T be what I may call the factor of torsion, for the particular quality of wire in use. By this I mean the force, measured in grains (provisionally), which, exerted at a distance unity (in inches) from the axis, will balance the tendency of the wire, when twisted through one turn to each unit of length, to untwist. Then, under the actual circumstances, the torsion of the pair will be expressed by

$$2\eta \cdot \frac{1}{2\pi} \cdot \frac{T}{R};$$

which must be divided by r to denote its power, applied at the ends of the wires, to turn the system.

Now, let P be released, so that this torsion may act upon it. Let $\eta = \theta + \phi$; then ϕ will take the place of η in the last expression, and the forces in opposition will be

$$\frac{P}{R} r \sin\theta, \quad \text{and} \quad \frac{1}{\pi r} \cdot \frac{T}{R} \cdot \phi;$$

and θ will obviously increase and ϕ decrease until there is equilibrium. Hence the approximate statical equation of the torsion-balance

$$Pr \sin\theta = \frac{1}{\pi r} T\phi,$$

$$\text{or } \sin\theta = A\phi, \quad \text{if } A = \frac{1}{\pi r^2} \cdot \frac{T}{P}.$$

Here A is an instrumental constant, which defines the relation of the parts θ, ϕ, of the whole angle η. As η increases, θ ϕ increase in the proportion $\frac{d\phi}{d\theta}=\frac{1}{A}\cos\theta$. Suppose a point reached where ϕ ceases to increase: then $\frac{1}{A}\cos\theta=0$, and $\theta=90°$. At this point, any increase of η is wholly absorbed by θ; which is easily understood, because at this point the bifilar opposition to torsion is at its maximum. Next, suppose η to be further increased, until the rate of decrease of ϕ equals the rate of increase of θ; in other words, to the utmost consistent with the relation between these components. At this point, $\frac{d\phi}{d\theta}=\frac{1}{A}\cos\theta_0=-1$, or $\cos\theta_0=-A$. At this point, too, θ ϕ can vary reciprocally without affecting η. It is a position which can only incorrectly be described as one of unstable equilibrium; for though the weight would not return if moved *forward*, but on the contrary, would continue to move forward, under the pressure of torsion; yet, if moved *backward*, it would seek to return. It is therefore stable on one side, and unstable on the other.

It is easy to test this practically. I have done so, and recognise, in the peculiar conditions, such as are well suited to afford an exact determination; as I will explain presently. But there are one or two theoretical points to be first noticed.

Since $\cos\theta_0=-A$, it follows that $\phi_0=-\tan\theta_0$; that A must be made not greater than unity; and that θ_0 must lie between 90° and 180°.

The condition that P must be $>\frac{1}{\pi r^2}T$ is an important one. Whenever this is the case, there *must* be a value of θ, at which the peculiar equilibrium will occur. The condition may be fulfilled in a way to make the position unsuitable for exact observation, but it nevertheless exists; whereas, if P is too small, there is no such position.

Transposing, we see that—

$$r^2 \text{ must be } >\frac{1}{\pi}\cdot\frac{T}{P},$$

from which it follows that whatever be the strength of the wires, or quantity of the weight, it is always possible, by modifying the distances between the former, to secure the necessary condition.

From this point of view, too, it appears that the length of the wires is immaterial. The opposing forces are equally affected by a change of length. The magnitude of R therefrom has nothing to do with the equilibrium.

Let us now work out a case. I find that a piece of pianoforte wire, 0·03 inch diameter, will carry 100 lbs.; will bear, without apparent

alteration, being twisted once in 6 inches; and has a factor of torsion equal to 23,700 grs.

Suppose we decide to use a weight of 200 lbs., so that $\frac{T}{P}=\frac{23,700}{1,400,000}=\frac{1}{59}$ nearly. Then r^2 must be greater than $\frac{1}{59\pi}$, or $r>\frac{1}{14}$ inch. This is so small a limit, that it affords no guide to the best value to give to r.

$$\text{Again, since } \phi=-\tan\theta=-\sec\theta\sqrt{1-\cos^2\theta}$$

$$=+\frac{1}{A}\sqrt{1-A^2}=59\pi r^2\sqrt{1-\frac{1}{\phi^2}} \text{ nearly,}$$

it is clear that r must be small, otherwise ϕ would be large, and this would demand a great length of wire. We may, indeed, put $R=6\frac{\phi}{2\pi}$ as the least allowable, and this at once gives $R=177\ r^2$, at least. It seems therefore that we must estimate R first. Let us take 3 feet or 36 inches. This gives $r<\frac{6}{\sqrt{177}}$, or $<\cdot45$ inch. A smaller value would give a lower pitch to the twist, which is very desirable. We may therefore take $r=0\cdot4$ inch. Whence $\phi=59\pi\times\cdot16$, in terms of radius, or $=\frac{59}{2}\times\cdot16=4\cdot7$ revolutions. Finally, $\tan\theta=-\phi$, and $\therefore\theta=92°$ nearly.

We may sum up the results, so far, as follows:—A weight of 200 lbs., suspended by two 0·03 inch steel wires, 36 inches in length, parallel at four-fifths of an inch apart, will be in unstable equilibrium at 92° from its normal position, when the upper ends of the wires have each been turned about their own centres through 4 revolutions 250°+92°, or 4 revolutions 342°, from the same normal position.

In this condition any addition to the weight will tend to strengthen the equilibrium, while, on the contrary, any relief of weight will throw the system over the summit and set it off, allowing it to expend the torsion in twisting the two wires together.

I will now show how this arrangement may be adapted as a gravimeter, not so much aiming at a description of a finished instrument as at a sketch of something which will fully illustrate what a more perfect design would fulfil.

A piece of wire, of the kind described, 7 feet long, is to be twisted upon itself, without straining, giving it ten full turns of torsion, and the ends are then to be secured to each other. This can easily be done so as to give it no chance of relieving itself otherwise than by *set*, a term which I understand to be the technical one in this connexion, meaning that change which takes place in a wire when it is supertorted or overturned beyond what its elasticity will bear. A wire so

tied up should retain its torsion indefinitely. The force thus permanently stored represents the constant force against which gravity is to be measured.

To use it, I imagine the ends of the double or twisted cord clamped in two frames of suitable make. The upper one grasps the junction, the lower one the bight. The wires issue from the jaws of the clamps at 0·8 inch from each other. The upper clamp is attached to a support capable of sustaining, without shake, the intended weight of 200 lbs. The lower is attached to the weight so as to have as little loose motion as may be, but so as also to be easily detached for transport. Before detaching it the wires should be allowed to twist upon each other. They would then be placed, with their clamps, in a box specially prepared to guard them from all imaginable injury.

There would be an arrangement above by which, when attached, the upper clamp would be raised slowly, so as to take up the weight; which would be turned without rising as the wires untwisted, until finally, it would hang; the wires then being in a state of torsion, the lower ends in a different vertical plane from the upper, according to the weight. I assume that on thus commencing the weight is in excess, so that the angle between the two planes (which will be the θ of the theory) will be about 90°.

In this condition we want means of relieving the suspension of *so much of the weight* as will allow the mass to reach the position of unstable equilibrium, and of determining *exactly how much.* This I propose to effect as follows:—

The weight is to consist of a cylindrical drum, capable of holding 3 cubic feet of water. Near the bottom it will have a small stopcock. When the apparatus is in repose, the cock is to be turned. The flow of water will relieve the weight, the drum will slowly turn, under the solicitation of the wires. If allowed to run freely it can be shut off when the critical point has been passed. This is the first approximation. A little water is put back, and now the cock is turned so as to reduce the flow to drops only. The effect can be watched with a microscope. The flow can at any time be stopped or accelerated. Experience will enable this determination to be made with a degree of precision which it is impossible to over-estimate.

The drum should be as light as possible, and silvered inside or platinised. The object is to secure a minimum of friction. As the critical point is approached the force is so small that it would take long to move the whole mass if solid; and if the whole mass were in motion its momentum would be troublesome. For the same reason the observation cannot be made until the body of water is at rest.

The same drum will serve for all weights, and makes it possible to obtain a series of results with different pairs of wires—all prepared in the same way, but of different torsions and widths apart.

Finally, the desired result will depend on the weights. I consider that to weigh accurately even a large body of water is a thing that can certainly be done, but how to do it forms no part of my present design. The drum might conveniently be constructed with a false bottom, the real bottom of the water space being conical, with the stopcock at the apex.

By an easy arrangement the closing of the cock might be effected as a consequence of the critical point of unstable equilibrium being reached and passed, the swing being checked by a stop pressing on the cock and shutting it off. Being thus self-acting, the flow might be made as slow as desired, with the certainty of the proper result being reached without further attention.

The quantity of water remaining could then be measured at leisure.

The change of gravity which would cause a change of rate of one second per diem in a seconds pendulum is $\frac{1}{43200}$ part. On a weight of 200 lbs., or 1,400,000 grs., this would be 32·4 grs., or rather more than half a fluid dram.

I am, of course, quite aware that the efficiency of such a gravimeter depends ultimately on the constancy of the torsion. This is a *sine quâ non* in any gravimeter on the torsion principle, and I assume it to exist. If it does not exist, no gravimeter on this principle *can* be successful.

Nor am I regardless that this, and all other gravimeters depending on elasticity, are at the mercy of temperature. But however impossible it may be to command a definite and uniform temperature wherever such an instrument may be instated for observation of the change of gravity, the converse is possible enough: the constancy of gravity can be guaranteed* while observations are instituted for determining the effect of thermometric change. So that if insufficient knowledge of the variation of elasticity with temperature debars such an instrument from being used as a gravimeter, it may with the more reason be looked to to increase that knowledge.

It seems only necessary to add that the mode of utilising the fact of a position of unstable equilibrium existing, if desired, between $\theta=90°$ and $\theta=180°$, as here indicated, may be open to objection without preventing that fact being capable of utilisation in some better way. The need of a statical gravimeter is so great that I hope what I have said may draw attention to the subject by showing how *very* simple it really is. Under correction, I would hazard the assertion that there is no instrument which would command a more immediate field of usefulness than a simply constructed gravimeter, and I think I have shown that it is possible to construct one.

* The contrary at any rate is, at the most, suspected more or less strongly, by a few.

II. "Note on the Microscopic Examination of some Fossil Wood from the Mackenzie River." By C. SCHRÖTER, Assistant at the Botanical Laboratory of the Polytechnic Institution, Zürich. Communicated by ROBERT H. SCOTT, F.R.S. Received October 22, 1880.

In the summer of 1880, Professor Oswald Heer transmitted to me seven specimens of fossilized wood from the Miocene beds of the Mackenzie River for microscopical examination, and, if possible, determination of the species, &c. Professor Heer's determination of the leaves and other remains of the flora of the locality in question have already appeared in the "Proc. Roy. Soc.," vol. 30, p. 560, having been communicated to the Society at the end of the last session.

The following are the species which I have been able to determine:—

1. *Sequoia Canadensis*, Schröter n. sp. (Specimens 1 and 2).*—This wood is very well preserved. (It belongs to the group *Cupressoxylon* of Kraus.) Its anatomy shows a great resemblance to that of *Sequoia gigantea*. The principal differences are the following:—

(*a*.) In the cells of the medullary rays of *S. Canadensis*, the radial pores are always arranged in one horizontal row, whilst *S. gigantea* has two rows of pores in the extreme ranges of cells.

(β.) The number of superposed ranges of cells in the medullary rays in *S. Canadensis* is 76. In *S. gigantea* it is only 55.

The differences between *S. Canadensis* and *S. sempervirens* are greater than those between the former and *S. gigantea*, so that *S. Canadensis* cannot belong to *S. Langsdorffii*, which occurs at the same locality, and which is the Tertiary ancestor of *S. sempervirens*. It is, therefore, probable that the wood which I have examined belongs to one of the other Tertiary species of *Sequoia* (perhaps *S. Sternbergii*, which approaches very closely *S. gigantea*). Until this identity is established I designate it provisionally as *S. Canadensis*.

2. *Ginkgo spec.* (Specimens 4 to 7.) Although not so well preserved as the preceding instances of wood, these specimens are easily recognisable as belonging to the genus *Ginkgo* by their very characteristic medullary rays. Perhaps they should be placed under the species *G. adiantoides*, Ung., which is the commonest of Tertiary *Ginkgos*, and has been found in Saghalien and in Greenland, between which two localities the Mackenzie River lies.

3. *Platanus aceroides*, Gp. (Specimen No. 2.) A dicotyledonous

* All the specimens referred to in this paper are to be deposited in the British Museum.

wood, which in the arrangement of its vessels and medullary rays resembles so closely the genus *Platanus* that it most probably is *P. aceroides*, which occurs at Mackenzie River in the leaf beds.

More complete details of my investigation will be found in my paper on the "Fossil Woods of the Arctic Regions," in the forthcoming volume (VI) of the "Flora Fossilis Arctica," by Professor Heer.

III. "The Electrostatic Capacity of Glass." By J. HOPKINSON, M.A., D.Sc., F.R.S. Received November 3, 1880.

(Abstract.)

In 1877 I had the honour of presenting to the Royal Society* the results of some determinations of specific inductive capacity of glasses, the results being obtained with comparatively low electromotive forces, and with periods of charge and discharge of sensible duration. In 1878 Mr. Gordon† presented to the Royal Society results of experiments, some of them upon precisely similar glasses, by a quite different method with much greater electromotive forces, and with very short times of charge and discharge. Mr. Gordon's results and mine differ to an extent which mere errors of observation cannot account for. Thus, for double extra dense flint glass I gave 10·1, Mr. Gordon 3·1, and subsequently 3·89.‡ These results indicate one of three things, either my method is radically bad, Mr. Gordon's method is bad, or there are some physical facts not yet investigated which would account for the difference. Two possible explanations have been suggested: 1st, possibly for glass K is not a constant, but is a function of the electromotive force. 2nd. When a glass condenser is discharged for any finite time, a part of the residual discharge will be included with the instantaneous discharge, and the greater the time the greater the error so caused. To test the first I measured the capacity of thick glass plates with differences of potential ranging from 10 to 500 volts, and also of thin glass flasks between similar limits; the result is that I cannot say that the capacity is either greater or less where the electromotive force is 5,000 volts per millimetre than where it is $\frac{1}{2}$ volt per millimetre. The easiest way to test the second hypothesis is to ascertain how nearly a glass flask can be discharged in an exceedingly short time. A flask of light flint glass was tested; it was charged for some seconds, discharged for a time not greater than $\frac{1}{17000}$ second, and the residual charge observed so soon as the

* "Phil. Trans.," 1878, p. 17.
† "Phil. Trans.," 1879, p. 417.
‡ "Report of British Association," 1879.

electrometer needle came to rest; the result was that the residual charge under these circumstances did not exceed 3 per cent. of the original charge, also that it mattered not whether the discharge lasted $\frac{1}{17000}$ second or $\frac{1}{80}$ second. These experiments suffice to show that neither of the above suppositions accounts for the facts.

I have repeated my own experiments with the guard ring condenser, but with a more powerful battery, and with a new key which differs from the old one inasmuch as immediately after the condensers are connected to the electrometer they are separated from it. In no case do I obtain results differing much from those I had previously published.

Lastly, a rough model of the five plate induction balance used by Mr. Gordon was constructed, but arranged so that the distances of the plates could be varied within wide limits. So far as instrumental means at hand admitted, Mr. Gordon's method was used. A plate of double extra dense flint and a plate of brass were tried. In the first, by varying the distances of the five plates, values of K were obtained ranging from $1\frac{1}{4}$ to $8\frac{1}{4}$, with the latter values from $\frac{1}{10}$ to 3. It is clear that the five plate induction balance thus arranged cannot give reliable results.

The explanation of the anomaly, then, is that the deviation from uniformity of field in Mr. Gordon's apparatus causes errors greater than anyone would suspect without actual trial. It is probable that the supposed change of electrostatic capacity with time may be accounted for in the same way.

IV. "The Cochlea of the *Ornithorhynchus platypus* compared with that of ordinary Mammals and of Birds." By URBAN PRITCHARD, M.D., F.R.C.S., Aural Surgeon of King's College Hospital. Received November 9, 1880. Communicated by Professor HUXLEY, Sec. R.S.

(Abstract.)

General Form of the Cochlea of the Duckbill or Ornithorhynchus.

This cochlea consists of a somewhat curved tube, about a quarter of an inch (6·3 millims.) in length, and one-twentieth of an inch (1·26 millim.) in diameter, projecting forwards from the cavity of the vestibule and embedded in the substance of the petrous bone. It is nearly horizontal, and is slightly curved outwards.

In section the tube is first oblong, with its greatest diameter from top to bottom, then somewhat triangular, and finally oval, with its greatest diameter from side to side. It terminates in a slightly enlarged rounded extremity, flattened from top to bottom.

Comparison with Typical Mammals and Birds.

In general form the duckbill's cochlea closely resembles that of the bird, and is very different to the spiral cochlea of the ordinary mammal. The two first differ, however, in that the duckbill's is more curved, and curved outwards instead of inwards, as in the bird. The enlarged apex of the former is rounded, that of the bird oval. The typical mammalian cochlea tube differs from that of the duckbill, in being spiral instead of merely curved, in tapering from commencement to apex, and in being much longer. Lastly, the axis of the spiral cochlea is horizontal, whereas that of the curved one is vertical.

The Internal Arrangement and Minute Structure of the Duckbill's Cochlea.

The interior of the tube is divided horizontally into two scalæ by a partition, the inner portion of which is thick and bony (lamina ossea); the outer, thin and membranous (lamina membranacea); a third scala is formed by a delicate membrane (membrane of Reissner) proceeding from the upper surface of the lamina ossea to the inner* wall of the tube.

The upper and larger division is the scala vestibuli, and this communicates posteriorly with the vestibule; the lower is the scala tympani. These two are united at the apex of the cochlea by means of an oval opening, helicotrema. The third, a small triangular tube, is the ductus cochleæ, or scala media, and this constitutes the membranous labyrinth; it contains the endolymph, and is entirely separate from the other two scalæ, which contain the epilymph. The ductus cochleæ is lined with epithelium; the scalæ vestibuli and tympani with endothelium.

The lamina ossea is a wedge-shaped mass of modified bone attached to the lower part of the outer wall and the outer part of the floor of the tube. Its inner free margin presents a deep groove (marginal sulcus), the lower lip of which projects further inwards than the upper. The lamina ossea does not extend to the apex of the cochlea, and thus allows of the communication between the scalæ vestibuli and tympani.

The ductus cochleæ is triangular in section, the floor is formed by the inner portion (limbus) of the lamina ossea and a strong membrane (membrana basilaris), which stretches from the lower lip of the sulcus to a mass of connective tissue (ligamentum cochleæ) adherent to the inner wall of the cochlea. The inner wall of the ductus is formed of this ligamentum cochleæ; and the outer wall, or sloping roof, by the delicate membrane of Reissner, which springs from the upper surface

* In describing the position of the parts in the duckbill's cochlea, the median line of the body is taken as the centre; in the spiral cochlea, the modiolus or axis.

of the limbus, and is attached to the upper part of the ligamentum cochleæ.

The membrana basilaris is composed of three fibrous layers; the lower, longitudinal; the middle, transverse; and the upper, formed of very fine transverse fibres. There are two blood-vessels running longitudinally in the lower layer.

The ligamentum cochleæ is a somewhat triangular mass of connective tissue with numerous blood-vessels, which in its upper portion run longitudinally, and, with the epithelium, form the stria vascularis.

The membrane of Reissner is composed of a delicate basement membrane, with the endothelium of the scala vestibuli on its upper surface, and epithelium on its under surface; here and there blood-vessels may be traced on it, running from the limbus to the ligament, and in some places these vessels form convoluted knots.

The epithelium lining the ductus cochleæ varies according to its position: that lining the membrane of Reissner is composed of a single layer of hexagonal cells; in the sulcus they are rounded; on the inner part of the membrana basilaris and the lower portion of the ligament they are cuboid; on the upper part of the ligament they are very peculiar, and resemble the transitional variety closely packed together. In the deeper part of the layer run numerous longitudinal blood-vessels, and this forms the already mentioned stria vascularis. The remaining portion of the epithelial layer that lies on the lower lip of the sulcus, and on the outer portion of the membrana basilaris, is developed into the so-called organ of Corti.

This organ of Corti consists of a double row of rods (of Corti), united at their upper ends and separate below; they stand on the membrana basilaris, and with it form a triangular tunnel.

The rods of both rows have cylindrical shafts and enlarged extremities; the upper ends of the inner row are rounded, and fit in corresponding concavities of the outer row. A delicate process projects inwards from the upper part of each of the rods, the processes of the outer ones lying above those of the inner.

Rows of hair cells are arranged on either side of these rods—one* to the outer side and three to the inner. Below the three inner rows are situated rows of nuclear cells (cells with well-marked nuclei, but no regular cell-wall), the cells of Deiters. Lying on the lower lip of the sulcus is a small mass of nuclear cells, and there is a row of these cells at each of the lower angles of the triangular tunnel.

The inner and outer side of the organ of Corti is formed of modified columnar cells. A reticulate membrane covers the rods and hair cells, the hairs of which project through certain circular meshes of the membrane.

* Since presenting this communication I have discovered a second row of hair cells in this position.

Covering the limbus, crossing the sulcus, and covering the organ, is a mucoid layer, the membrana tectoria.

Nerve filaments pierce the upper lip of the sulcus and pass to the hair cells and nuclear cells of the organ.

The organ of Corti, with the membrana basilaris below and the membrana tectoria above, form the lamina membranacea.

The ductus cochleæ commences as a delicate tube, no doubt connected in some way with the saccule of the vestibule. Its termination is very peculiar; instead of ending with the lamina ossea, where the organ of Corti ends, it is continued round the apex of the cochlea to form three-fourths of a circle. Just past the end of the lamina it forms a circular tube; at the other side of the apex, it enlarges into an oval chamber (lagena) which terminates at the base of the lamina ossea. This lagena is lined by epithelium, chiefly cuboid, but with one large patch of nerve epithelium, like the thorn cells and bristle cells found in the maculæ acusticæ of the vestibule. ("Quar. Jour. of Micros. Science," p. 379, 1876.)

The cochlear branch of the auditory nerve passes through the bone on a level with the floor of the tube, but to its outer side. It gives off lateral branches all along to the lamina, and its terminal fibres go to the lagena. The lateral branches pass through a ganglionic mass, similar to the ganglion spirale, and then on through the lamina, close to its lower surface, finally perforating the upper lip of the sulcus by a single row of holes (habenula perforata) and entering the organ of Corti as already described.

Comparison of the Minute Structure of the Cochlea of the Duckbill with that of Typical Mammals.

From the foregoing description, the duckbill's cochlea is shown to be so unmistakably mammalian in type, that merely the differences will here be noted.

The lamina spiralis membranacea increases in width, and so do its component parts, from base to apex of the spiral cochlea; in the duckbill's this widening takes place, but not nearly to such an extent as in the spiral cochlea.

The rods of Corti in the duckbill are not so well developed as in the typical mammal.

The membrane of Reissner in this monotreme presents blood-vessels on its surface with convoluted knots; these I have never found nor read of in this situation in any other mammal. The vas spirale of the ordinary mammal is represented by two vessels in the duckbill.

The course of the cochlear nerve necessarily differs in the two forms of cochlea.

But the great difference is found in the presence of the lagena at the

end of the duckbill's ductus cochleæ; this has never been found in mammals, but is found in birds, reptiles, and amphibians.

Comparison with the Bird.

A brief description of the bird's cochlea, will be found in my paper, *in extenso;* in this abstract I propose only noting the similarities and dissimilarities.

The scalæ tympani in each type of cochlea correspond.

There is no scala vestibuli in the bird, the scala media (ductus) occupying the whole of the upper division of the tube.

The membrane of Reissner and stria vascularis is represented by the tegumentum in the bird.

The lamina ossea corresponds to the quadrilateral cartilage, and the ligamentum cochleæ to the triangular cartilage of the bird.

There are no rods of Corti in the bird: the hair cells are more numerous and their component hairs are united together into a spine.

The nerve fibres pierce the quadrilateral cartilage by numerous rows of holes, instead of one row, as in the duckbill and other mammals.

The lagena, with its macula acustica, is found both in the bird and duckbill, but in the former is a direct continuation of the ductus, whereas in the latter it is connected by means of a constricted tube. Moreover, the ductus of the duckbill makes three-fourths of a turn, but that of the bird is nearly straight.

General Conclusions arrived at by the Research.

Although the outer form of this monotreme's cochlea resembles that of the bird in being nearly straight, yet its internal arrangement is decidedly mammalian.

The general acoustic apparatus of the duckbill's cochlea is not nearly so extensive as that of the ordinary mammal nor is its organ of Corti so well developed.

Lastly, the duckbill's cochlea possesses an addition, the lagena, which is not found in any other mammal, but which is found in the bird, reptile, and amphibian. Thus it presents a distinct link between the cochlea of the higher mammals and that of the lower vertebrates.

In conclusion, I desire to give my most hearty thanks to the many Australian friends who, by their zeal in my cause, have provided me with specimens of the ornithorhynchus in such a good state of preservation as to allow of their microscopic preparation and examination.

December 16, 1880.

THE PRESIDENT, followed by Dr. C. W. SIEMENS, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On Actinometrical Observations, made in India at Mussooree and Dehra in October and November, 1879. By J. B. N. HENNESSEY, F.R.S., Deputy Superintendent, Great Trigonometrical Survey of India. Received May 4, 1880 (the parts within square brackets August 28).

The present actinometrical observations were taken in 1879, in continuation of the series of 1869, published in the "Proceedings of the Royal Society," vol. 19 (1870), pp. 225—234; reference is suggested in these pages for such particulars as are omitted here in order to avoid needless repetition. The series of 1869 and 1879 were taken under identical circumstances as respects stations of observation, actinometers, and observers; in both instances the work was carried on only in the entire absence of visible cloud or mist between the sun and the observer, and the sun's declination during the measurements of 1869 was nearly the same as in the observations of 1879. Hence the two series, separated by an interval of ten years, are thus highly eligible for comparison with one another; it may, however, be added that the work of 1879, now under discussion, is more extensive and systematic than that of the preceding series.

2. The two actinometers used are of the kind invented by the Rev. G. C. Hodgkinson, and described by him in the "Proceedings of the Royal Society," vol. 15, p. 321. The instruments are the property of the Royal Society. To distinguish the instruments from each other, I marked one with the letter A and the other with B, in 1869; both are in exactly the same original good condition, including their glass caps, and I may add in particular that the same glasses, and in the same positions, were used in 1879 as in 1869. Hence the constants determined and employed in 1869 were also employed on the present occasion; these constants are as follows:—

Factor No. 1 for actinometer A, to convert results with *glass on* into results *glass off* .... 1·09

Factor No. 2 for actinometer B, to convert results with *glass on* into the results *glass off* .... 1·04
Factor No. 3, to express results obtained with actinometer B *glass on* in terms of actinometer A *glass on*.. 0·982

All the results given in Tables V to VIII, attached, will be found expressed *in terms of A* for both *glass on* and *glass off*, as was done in 1869; it will be seen that only factors Nos. 1 and 3 enter into this conversion. The results of Table IX need no such conversion, as for the purpose in view they are compared for each instrument with *its own* mean. The numbers hereafter discussed are all in terms of A *glass off*.

3. The co-ordinates of the stations of observations are as follows:—

	Lat. N.	Long. E.	Height in feet above mean sea level.
Mussooree	30° 28′	78° 7′	6,937
Dehra..........	30° 19′	78° 6′	2,229

The direct distance is thus about nine miles between the stations, which are nearly on the same meridian and are mutually visible. The difference of longitude between the two stations is only 62″·5=4·2 seconds, Mussooree being east of Dehra.

4. Dehra station is in the Dehra Dun, which is a valley some ten miles wide and about forty miles long, and is bounded on the north by the Himalayas and on the south by the Siwalik range of hills; at its eastern and western extremities there flow respectively the well known rivers Ganges and Jumna. The large native town and the civil and military stations of Dehra lie west and north of the actinometer station within a distance of some two miles, so that the observer at Dehra was unavoidably subject to the disadvantages of the usual smoke and haze envelope which is commonly visible in the winter months over all large towns when seen from a distance, and especially if viewed from a height; on the other hand, the observer at Dehra was not liable to the disadvantages of strong winds.

5. Mussooree is eminently suited for an actinometric station, especially in the autumn. It stands on almost the highest point of an east and west ridge of hills, which falls precipitately 3,000 or 4,000 feet both north and south, so that the observer is absolutely free from smoke and dust, while the atmosphere in autumn is brilliantly clear. But the station from its very prominence is liable to brisk breezes, which sometimes blow in strong gusts and always from the south. From these, however, the observer and instrument were protected by means of a kanát (or canvas wall, some six feet high, of a tent), erected at a distance of a few feet, a protection found to be absolutely necessary, for the actinometer is certainly affected by a breeze, especially if blowing in gusts.

[It will thus be seen that while Mussooree is highly eligible, a similar

claim cannot be advanced for Dehra. Further, it may be objected that the distance between the two stations is not sufficiently great, being only some nine miles. To this can be urged, that in view of the latitude and the sun's declination, the direct rays to the sun from the two stations, about noon, travelled nearly seven miles apart from one another through the envelope of the earth's atmosphere: whether this be sufficient or not, is perhaps not so readily apparent. But as a matter of fact, the time at our disposal did not permit of our observing anywhere *but at Mussooree and Dehra*, so that the choice lay between accepting Dehra or dispensing with a second station. Notwithstanding the drawbacks to Dehra individually, its results appear less liable to distrust if in accord than if in conflict with those at Mussooree: for the latter case visible causes are not wanting, but the former is difficult to account for without assuming special conditions. To this may be added, that distinct mutual visibility between the stations, and the ability at Mussooree to look nearly a mile above Dehra, are not without advantages.]

6. The observations with B at Dehra were taken by Mr. W. H. Cole, M.A., those with A at Mussooree and Dehra by myself.* The procedure prescribed in the "Admiralty Manual of Scientific Enquiry," pp. 129—130, was exactly followed (as was done in 1869), whereby the change of reading in 60 seconds is obtained alternately in sun ($\odot$) and shade ($\times$) at intervals of 30 seconds (beginning and ending with a $\odot$ observation;† hence in a series of measurements there result $(n+1)$ of the latter to n of $\times$. Ordinarily the average result from a series is found from

$$R=\frac{1}{n+1}[\odot]_1^{n+1}+\frac{1}{n}[\times]_1^{n}\ddagger$$

where R stands for the mean radiation, and the brackets [ ] denote summation. We may, however, exhibit the successive values of radiation for 60 seconds by writing

$$R_1=\frac{1}{n}\left[\left(\frac{\odot_1+\odot_2}{2}+\times_1\right)+\left(\frac{\odot_2+\odot_3}{2}+\times_2\right)+\ldots+\left(\frac{\odot_n+\odot_{n+1}}{2}+\times_n\right)\right]$$

hence
$$R-R_1=\frac{1}{n}\left\{\frac{\odot_1+\odot_{n+1}}{2}-\frac{1}{n+1}[\odot]_1^{n+1}\right\};$$

This difference is very small, especially if the series be long continued, so that n is large. Accepting the mean value R_1, its individual results may be conveniently reckoned as if obtained at the mean of the beginning and ending times of successive *shade* observations; thus the pth result, or

* Excepting on 1st and 2nd November, when Mr. H. W. Peychers observed.

† See Table IX for example.

‡ Corrections to 32° Fahrenheit being understood.

$$R_p = \frac{\odot_p + \odot_{p+1}}{2} + \times_p,$$

would be tabulated as if corresponding to the mean of the beginning and ending times of $\times_p$; no other times need be entered in the table of individual results, which may thus be exhibited in a compact and simple form.

7. Remembering that the actinometer, as at present constructed, is a *differential* instrument, it appears desirable to regard the causes, to which the measured changes are due, under separate heads, which may be briefly and generally indicated by

Instrumental,
Local, and
Intrinsic or Residual.

[These three "heads" may be more definitely expressed symbolically. If V stands for the measured value of change in solar radiation, suppose

$$V = S + (L + l) + (I + i)$$

where S stands for that part of V due to true or unmodified solar energy, a quantity which appears unattainable *by itself*, at least for the present: $(L+l)$ and $(I+i)$ are errors by which S may be vitiated, so as to become V. Of these errors, let $(I+i)$ stand for the portion due to the instrument and its manipulation, such that I can practically be controlled and eliminated, while i may imperceptibly exist *in combination* with S: also let $(L+l)$ denote all the errors appertaining to the locality, *i.e.*, *between the sun and the instrument*, during the minute of observation, such that L, arising from visible or otherwise detectable causes, can be excluded, while l, being invisible and beyond endeavours to evade, may also imperceptibly exist *in combination* with S. *Then* I and L stand respectively for the errors above indicated by "Instrumental" and "Local." Now, if by skill, care, and vigilance, we succeed in making $L=0$ and $I=0$, and obviously actinometrical observations are worthless unless there be reasonable prospect of securing these conditions, then there remains, *as the residual of* V, the quantity

$$V_1 = (S + l + i),$$

but since by definition l and i are not visible, nor yet detectable, their presence or absence, in a given result, if generally suspected, can neither be positively affirmed nor denied, appreciably speaking, nor yet estimated. For their presence I use, as already said, the designation V_1 (*i.e.*, the residual of V); in their absence, when $l=0$ and $i=0$, the designation "intrinsic" seems suitable, for now V_1 becomes $=S$. Hence arose the double designation, adopted above, of "intrinsic or residual," whereby provision is made for either assumption, *without an*

attempt to assert which prevails: so that, *in the sense here employed*, residual and intrinsic (or other equivalent phrase) are convertible terms, as *is intended throughout this paper*. This admission need not, I think, be received as a deterrent to the use of the actinometer under proper conditions, for when we know more of V_1 the road to S may become easier. Meanwhile, as a matter of fact, the actual radiation by which we are normally affected, at least in sunny lands, is represented, perhaps, more nearly by V_1 than by S.

It may also be stated here, once for all, that the conclusions advanced in this paper, like most others of their kind, are not intended to cover more ground than the observations themselves; the former go, of course, no further than the latter.]

Of these the residual effect alone presents the real object of measurement. Complete elimination of instrumental defects, with retention of excessive sensitiveness, is still a desideratum, and local causes may not only produce overwhelming effects,* but they are so completely beyond estimation or control, that the only remaining alternative is never to observe when they are visible or likely to be present, at any rate if small quantities are the objects of search. As, however, the necessity for placing the actinometer, so far as practicable, beyond local influences, is now so fully recognised, I need only here dwell on one of the further conditions necessary to be secured. This suggests itself if only on the score of fallibility and the necessity to collect in general abundant evidence. On these grounds alone a series of observations should be prolonged as much as practicable, so as to yield numerous instead of only a few results. Here, however, apart from instrumental inadequacy, we are met under even the most favourable local conditions, by the varying absorptions of our own atmosphere at different zenith distances, and it thus becomes a matter of primary importance to establish a time-range, during which inconstancy of radiation from this cause may be reckoned as certainly absent, at least so far as the sensitiveness of the instrument can detect.† In fact, while the observations at varying zenith distances may eventually yield valuable results for reduction to the local zenith, it appears wiser in the first instance to set aside every complicating cause that can be avoided and to establish a daily time-range of the kind indicated.

* In course of the present observations I have arrived at the conclusion that the presence of strati, even if distant from the sun, sensibly affect the radiation, while cumuli are comparatively innocuous.

† The capability to measure minute changes, besides the power it confers, governs the duration of a single exposure, which in turn is the less likely to afford means of detecting short solar periodicities, the more numerous the latter are within the duration. No doubt a contrivance for yielding a *continuous* curve would leave nothing further to be desired in respect of periodicities, but I am not aware that this has as yet been achieved, without sacrifice of the essential sensitiveness, accuracy, and durability.

8. Accordingly, during the present series, this time-range was made one of the objects of inquiry; it would most probably be found to occupy an equal hour angle, + and − from the meridian, a point, however, which was itself included in the investigation, by providing that the observations should be made *continuous* for each day. Eventually, the hour angles adopted were 30^{m} E., and 30^{m} W., whereby 21 observations in ⊙ and 20 in × would be obtained, yielding ten results before, and as many after apparent noon, by the tabulation of Article 6; always provided that the observer missed none of the numerous readings, a result which requires some practice to secure in a series exacting continuous attention for so long a period as one hour. In addition to these daily time-range series, the observers were also to take a long range or hourly series (*i.e.*, at every hour, from 8 A.M. to 4 P.M.), on two days, each comprising six observations in ⊙ and five in shade, so arranged that the middle result should occur at *the* hour. The whole of the simultaneous results thus obtained at Dehra and Mussooree are tabulated according to Article 6 in Table V, and may be briefly explained, thus:—

9. Table V.—The times are the means of the times of × observations: they are given by preference in entire minutes, to avoid the needless statement of seconds; but the exact second, or local apparent time, if required, can be readily found by applying to the chronometer time the error in seconds given for each day; thus, on October 31st, the chronometer was fast on apparent time by 5^{s}; this error will always be found to be under $\pm 30^{s}$. The angle between the two local meridians is $4^{s}\cdot 2$, Mussooree being east of Dehra. The results in sun, or ⊙ (heat gained), and those in shade, or × (heat lost), are the observed results reduced to 32° Fahrenheit;* from these, for both A and B, there follows directly the radiation or ⊙ + × *glass on*, which, in the case of A, was converted, by means of factor No. 1, Article 2, into *glass off;* in the case of B, the factors Nos. 3 and 1, were successively employed to express in terms of A respectively *glass on* and *glass off*. Thus the whole procedure in reduction was exactly the same as that followed in 1869. Further, the means of the results before and after noon are given separately together with the estimate of accuracy indicated by their "mean errors;" these latter, it need hardly be stated, cannot recognise errors of a *constant* nature. The table also includes a record of the barometer, and of the thermometers, wet and dry in shade, and of black bulb in sun, together with an exact statement as to the wind and aspect of sky. In all, the observations at Mussooree include 392 in ⊙, and 361 in × ; those at Dehra, 244 in ⊙ and 217 in ×, unavoidably excluding at the latter station several taken on the first four days, because they were beyond

* By means of the table of expansion for alcohol by Kopp given in "Gmelin's Chemistry."

the time-range adopted. It will be found that the work was practically simultaneous. The radiation results hereafter discussed, are those in terms of A, *glass off*, the unit being a tenth of a millimetre, or about 0°·002 Fahrenheit.

10. Table VI.—To test the constancy of radiation during the time-range of $\pm\frac{1}{2}$ hour adopted, we might compare the successive *individual* results with one another in each half-hour, just as they stand in Table V; this comparison, however, would be burdened by errors of single values, which may be reduced with advantage. To secure this end, I subdivide each half hour into three similar sub-time-ranges, and within these limits I take the mean of all the available results for each day to form a group mean; hence, each daily series of group means will be six in number, provided the series was completely observed. The sub-time-ranges adopted, are—

	h.	m.		h.	m.
Before noon	11	30	to	11	40
	11	40	,,	11	48
	11	48	,,	0	0
After noon	0	0	,,	0	12
	0	12	,,	0	20
	0	20	,,	0	30

Now, each group mean is the mean of three to five individual results; the exact numbers of the latter will be found indicated by subscripts. Entering the group means in their proper columns, we obtain in the case of Mussooree, fourteen complete series (omitting the nineteenth, as it supplies only four of the six groups); and, taking the mean for each sub-time-range, we now find mean results from so many as from thirty-eight to sixty-one *individual* results. These mean sub-time-range values may be considered practically free from all variations, except those due to intrinsic or residual causes (Article 7); no doubt, daily instrumental *constants* are present, but as the latter will be eliminated in the proposed comparison, *inter se*, the results in question may be accepted as highly eligible, for the purpose of testing how far the radiation is constant within the $\pm\frac{1}{2}$ hour time-range.

11. As regards Dehra, the group results, from accidental causes already explained, present but three complete series. I pass on to notice the exhibits of Tables VII and VIII before entering on a brief discussion of all the facts.

12. Table VII.—This table also is compiled from Table V, and shows, daily, the sun's meridional zenith distance, and the mean values of radiation for each half-hour time-range, and for the day, together with readings of barometer and thermometers, and declarations as to the wind and aspect of sky at noon. Finally, individual and mean *differences* of radiation, M.—D. (*i.e.*, Mussooree minus Dehra), as well as the mean radiation at each station are deduced.

13. Table VIII.—Table V supplies the materials also for this table, which presents the mean facts of the hourly or long series, taken on the 12th and 14th November, simultaneously, at both the stations of observation. Each series furnishes nine mean results, of which the middle value is at noon. By comparing the radiation at noon with the hourly results for each day, we obtain the defect of radiation from noon corresponding to the change in the sun's zenith distance.

14. Now, collecting the results from Table VI for *Mussooree*, we find the mean sub-time-range values to be—

h.	m.		h.	m.		No. of results.		Mean group.	
11	30	to	11	40	....	42	....	957	
11	40	„	11	48	....	40	....	958	
11	48	„	0	0	....	55	....	957	mean 957.
0	0	„	0	12	....	61	....	953	
0	12	„	0	20	....	40	....	952	
0	20	„	0	30	....	38	....	949	mean 951

from which it appears :—

(1.) The radiation *is* sensibly constant for each half-hour.

(2.) The radiation, during the half-hour before noon, *exceeds* that prevailing during the half-hour after noon; in the present instance, the excess is only six-tenths of a millimetre, but notwithstanding the smallness of this quantity, it appears to be real, not accidental.

(3.) Apart from constants, it may be inferred from the accordance of the results that the instrument is susceptible of a high degree of accuracy.

Again, the above conclusion (2) is very decidedly confirmed by the results for Mussooree, from Table VIII, where the sums of the defects of radiation from the noon value furnish large test-measures of the point in question, and are as follows :—

	Nov. 12.		Nov. 14.
Sums for the four hours before noon	287	....	332
„ „ „ after noon	386	....	392
After noon—Before noon........	99	....	60

Mean (indicating excess of radiation before noon).... 79

15. The *Dehra* results in Table VI are not sufficiently numerous or symmetrical to test the conclusions (1) to (3) of Article 14; in fact, they present but three *complete* series of groups; nor may *incomplete* series be admitted in this test, because of the undeniable presence of variable daily constants. And with regard to Dehra evidence, in

Table VIII, there are obvious inconsistencies in these hourly results, such, however, as have been experienced on some other occasions with similar actinometers. No doubt, in point of eligibility as a station, Mussooree is very far superior to Dehra, as already shown, so that *small* fluctuations may be expected to disappear at the latter, through the dissipation of energy exacted by its denser envelope; but the sums of the defects present *large* measures of comparative radiation before and after noon, and while Mussooree gives a decided preponderance of radiation to the former period, this does not hold at Dehra, as may be thus shown:—

	Nov. 12.		Nov. 14.
Sums for the four hours before noon	346	....	432
,, ,, ,, after noon	346	....	420
After noon – Before noon	0	....	12
Mean (indicating excess of radiation before noon)		6	

This discrepancy in evidence may, however, be fully accounted for by local haze, which is certainly absent at Mussooree, and as undeniably is present at Dehra; accepting this conclusion, the observations show a fact not without interest, viz., that at the last-named station the local haze absorbs more heat relatively in the forenoon than in the afternoon, a conclusion which can be supported by other exhibits from the same table. And, as regards the inconsistencies in question, whereby the radiation in some instances *decreases* with *decrease* of zenith distance, these, also, may to some extent arise from passing local causes, such as are inevitable in the neighbourhood of a large town. But after all these points have been considered, there still remains the fact that separate "casts-off" of the instrument are likely to be burdened each with a constant error, by which the whole series may be affected.

16. But though ineligible in the form of group means (as given in Table VI) to test the conclusion (1) of Article 14, the evidence of the Dehra results may be legitimately employed when presented in the form given in Table VII, where the mean results are for $\pm$ half-hourly time-ranges. Thus we find

	Before noon.		After noon.		Before – After.
Mussooree mean of first eight days	954	....	947	....	7
,, ,, remaining seven days	966	....	956	....	10
Dehra mean of *complete* seven days	882	....	880	....	2

which are in accord and in keeping with the hypothesis advanced as to dissipation of energy.

17. The facts of Table VII are exhibited in a form suitable for comparison with the similar results of Table II of 1869.* Thus, for the difference of radiation between Mussooree and Dehra, or M—D, we have from the mean of all the results in each case

	M—D.
For 1869, from Table II....................	77
For 1879, from Table VII..................	74
Difference	3

where it will be remembered that M—D is independent of all variations which affected the two stations *equally at each epoch*. The result shows no change of *relative* radiation in 1879 as compared with 1869.

18. I next compare the radiation results *at each station* for the two epochs, and these, no doubt, are subject to all the indicated causes of change, some of which can be recognised, and even roughly estimated. In the first instance, however, I accept the results as exhibited, and compare them thus:—

Means of all the Results.

	Mussooree.	Dehra.
In 1869, Table II	980	902
In 1879, Table VII	954	877
Difference on D_1........	26	25

where the very close accordance of the two results suggests that they represent *intrinsic* or residual causes solely. The magnitude of D_1 is, however, liable to correction, from two causes,† because its components are not in the same terms; both the causes, however, affect the two stations equally.

Cause (*a*.)	In 1869 the time-range was	$\pm$ 1 hour.
	In 1879 „ „	$\pm \frac{1}{2}$ hour.
	Difference	$\frac{1}{2}$ hour.

* "Proc. Roy. Soc.," vol. 19, p. 229.

† A third cause (*c*) may also be noticed, viz., that presented by a change in the earth's radius vector; reckoning the effect of this inversely as the squares of the radius, there results a *percentage* of only 0·4 of the unit of radiation adopted in this paper, *i.e.*, one-tenth of a millimetre: this result is quite rejectaneous. It would tend, however, to increase the magnitude of D_1.

Cause (*b*.)	In 1869, mean of sun's meridional zenith distances (at Mussooree)............	44°·6
	In 1879, mean of sun's meridional zenith distances (at Mussooree)............	47°·3
	Difference	2°·7

hence, to make the results of 1869 strictly comparable with those of 1879, the former (or D_1) must be increased for (*a*) and decreased for (*b*). We might estimate these corrections empirically, proceeding on the basis afforded in Table VIII of the defect of radiation corresponding to $\pm$ 1 hour to + 2°·1 of sun's zenith distance. It appears, however, sufficient to note that the effects of (*a*) and (*b*) are of contrary signs, and that the excess of (*b*) may be set down roughly at about five units, whence D_1 would be reduced to about twenty, of which the principal portion, if not the whole, may be ascribed to intrinsic or residual variation. Hence, it may be asserted with confidence, on the evidence adduced, that the solar or residual radiation in 1879 was less than in 1869 by so much as 2 per cent. If we imagine this result to hold good for the twelve months, some notion may be received of its consequences by conceiving that, in point of heat-rays at least, the sun was abolished one entire week in 1879.

[To this may be added, that since the *larger* radiation of 1869 corresponds more nearly than the *smaller* radiation of 1879 to a period of *maximum* sun-spots, this result confirms, so far as the evidence goes, the hypothesis of greater solar energy at maximum than at minimum epochs.]

19. It will be seen in the column of *mean* results for the day, Table VII, that considerable differences appear on a comparison *inter se;* at the same time that the several *individual* results for the day (in Table V), of which the means are composed, exhibit comparative harmony. This leads to the conclusion that all the results for each day contain one or more constants; but it is by no means a simple matter to show by experiment* how these constants may be subdivided between intrinsic or residual and instrumental causes; as to local causes, they may be disregarded at least at Mussooree. That instrumental causes do exist I am strongly inclined to believe, though probably they eliminate one another in the mean from a large number of days; but supposing them to be real, these causes are in all likelihood connected with the preliminary adjustment of the fluid, by which a sufficient quantity of the latter is "cast off" into the surplus bulb above, so as to make the head of the column play up and down

* Some experiments have been made, but they are not sufficiently numerous to justify a conclusion.

within the range of the scale. In an instrument so essentially differential as the actionometer, any adjustment involving a change of circumstances cannot be too earnestly deprecated. On the other hand, without excessive sensitiveness, the instrument can command only a very limited range of utility.

20. There remain to notice a few experiments made in view of very short period fluctuations in the solar or residual radiation;* these are given in Table IX. On this occasion the two instruments were set up at Dehra within three or four feet of one another; both were read by means of the same chronometer placed between them, and the two observers capped and uncapped their tubes at the same times. Thus, the readings were taken at the same instant, and under *precisely* identical circumstances, the observations being continuous from 11 h. 39 m. to 0 h. 31 m. apparent time. Taking for each instrument the difference between each result and the mean of all, we obtain the values in columns dA and dB. Now, the magnitudes of these changes are undoubtedly in excess of fallibility in reading, for the eye can certainly read to one-fourth of a scale division (or less), *i.e.*, to $\frac{1}{4}$ millim., or about 0°·005 F., so that apart from calibration errors, which are not likely to be excessive, and which in the two instruments are almost certain to be *dissimilar*, the inference is that these large differences are at least in part due to solar or residual causes. This conclusion is numerically tested in the last columns of the table, where I have placed every pair of dA or dB, in which either member exceeds ten in magnitude irrespective of size; twelve such pairs occur, and it may be worthy of notice that the two members of *every* pair without exception are affected by the *same* sign; so that so far as the evidence goes, *both* instruments recorded a rise, or *both* recorded a fall. To present a large argument of the fact, I take the sums, disregarding signs, of each column, from which it appears that A recorded 151 units of change, and that B recorded no less than 138 units, and *in the same directions*. The result, so far as it goes, confirms the conjecture that the solar or residual radiation is remittent, and in very short periods of unknown duration.

[I cannot too earnestly express my hopes, that actinometrical observations will soon be begun on at least two of the several elevated and eminently eligible sites which India offers. A series conducted systematically and skilfully for even one twelvemonth could not fail to throw much light on a subject which, in its eventualities, concerns every individual on this globe far more than is perhaps commonly contemplated. As to the more immediate questions of periodicity, &c., these cannot be more than approached by means of desultory observations, such as those here presented, undertaken under the pressure of

* See "Proc. Roy. Soc.," vol. 19, p. 228, Article 12.

daily official duties, and with little prospect, though with hearty desire, to command time for more continuous and conclusive results.]

21. For support and encouragement in making these observations, my acknowledgements are due to Major-General J. T. Walker, C.B., R.E., F.R.S., Surveyor-General of India, whose appreciation of scientific inquiry is well known.

22. In concluding my remarks on the Mussooree-Dehra observations, I gladly avail myself of the opportunity to heartily thank my friend Mr. W. H. Cole, M.A., whose skill as a trained observer, and whose co-operation in general have enabled him to render valuable aid.

Observations by Captain J. P. Basevi, R.E.

23. It only remains to offer a few words on the results, exhibited in Table X, of actinometrical observations made by my friend the late Captain J. P. Basevi, R.E., of the Great Trigonometrical Survey of India.

24. When this officer, in course of the pendulum observations, was about to resume his travels in 1871, it occurred to us that he might utilise some rare opportunities for actinometrical observations which would present themselves at points of considerable height above sea level that he would certainly need to pass over. With his usual love for scientific research, Basevi readily consented to use the actinometer on suitable occasions, and he was accordingly provided by me with the instrument A of the Royal Society. The intention to observe at great heights was unhappily frustrated by his lamented death, or there is no doubt he would have secured valuable results at very considerable altitudes, including the Takalung La, or pass, on which he stood at 18,060 feet above sea level. He, however, took some preliminary observations at small heights, chiefly at Srinagar, the capital of Kashmir. Not being sufficiently numerous for a separate communication, they have remained in my hands since 1871, when they were taken, awaiting an opportunity, such as now presents itself, for placing them on record. This is done in Table X.

Table V.—Actinometrical Observations and individual Radiation Results in India, at Mussooree with Actinometer A, and at Dehra with Actinometer B, expressed in terms of A.

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32°.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
		⊙	×	Glass on	Glass off		In Shade. Dry.	In Shade. Wet.	In sun, black bulb.			⊙	×	Glass on	Glass off		In Shade. Dry.	In Shade. Wet.	In sun, black bulb.
	(Fast 5s)* h. m. s.	1879, October 31.																	
1	11 32 0	485	421	906	988	23·456	56·2	48·1											
2	35 0	472	438	910	992	Entire sky beautifully clear.													
3	38 0	460	458	918	1001														
4	41 0	447	450	897	978														
5	45 0	450	460	910	992														
6	48 0	482	440	922	1005														
7	51 0	461	447	908	990														
8	54 0	433	460	893	973														
9	57 0	443	443	886	966														
10	0 0 0	435	432	867	945														
	Mean...	457	445	902	983 ±1·5														
10	0 0 0	435	432	867	945														
11	3 0	424	461	885	965	Light breeze from S.													
12	6 0	405	490	895	976														
13	9 0	427	463	890	970														
14	12 0	444	443	887	967														
15	15 0	436	461	897	978														
16	18 0	446	441	887	967														
17	22 0	445	446	891	971														
18	25 0	439	443	882	961														
19	28 0	422	450	872	950														
	Mean...	432	453	885	965 ±1·0														

This and all other time errors are errors of the chronometer times on local *apparent* times; and it will be found that these errors are always less than ± 30s.

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In Shade.		In sun, black bulb.								In Shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 5s) h. m. s.	1879, November 1.																	
20	11 31 0	449	408	857	934	A few small patches from S. some 30° W. of sun, sky other-clear.													
21	34 0	464	395	859	936														
22	37 0	421	417	838	913														
23	43 0	407	458	865	943														
24	46 0	396	464	860	937														
25	49 0	403	474	877	956														
26	52 0	398	468	866	944														
27	55 0	432	470	902	983														
28	58 0	456	446	902	983														
	Mean...	425	444	869	948 ±2·2														
29	0 1 0	435	421	856	933	23·430	57·9	50·6											
30	4 0	436	447	883	962	Light breeze of light cloud wise quite													
31	7 0	441	433	874	953														
32	10 0	422	437	859	936														
33	16 0	383	473	856	933														
34	21 0	375	483	858	935														
35	24 0	396	483	879	958														
	Mean...	413	454	867	944 ±1·7														

At Mussoorie.										At Dehra.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In Shade.		In sun, black bulb.								In Shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 5s) h. m. s.	1879, November 2.																	
36	11 30 0	515	370	885	965														
37	33 0	503	392	895	976														
38	36 0	479	434	913	995														
39	39 0	462	430	892	972														
40	42 0	433	452	885	965														
41	45 0	417	461	878	957														
42	51 0	455	452	907	989														
43	54 0	450	448	898	979														
44	57 0	457	457	914	996														
45	0 0 0	468	435	903	984	23·400	57·4	48·1											
	Mean...	464	433	897	978 ±1·1														
45	0 0 0	468	435	903	984														
46	3 0	465	429	894	974														
47	6 0	455	453	908	990														
48	9 0	454	458	912	994														
49	12 0	461	423	884	964														
50	15 0	454	438	892	972														
51	18 0	465	431	896	977														
52	21 0	472	432	904	985														
53	24 0	462	426	888	968														
54	30 0	440	444	884	964														
	Mean...	460	437	897	977 ±1·0														

Gentle breeze from S. Entire sky beautifully clear.

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32°.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In Shade.		In sun, black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 5s) h. m. s.	1879, November 3.									(Slow 28s) h. m. s.	1879, November 3.							
55	11 32 0	502	365	867	945	23 ·384	56 ·8	48 ·2											
56	35 0	483	390	873	952	with occasional puffs. fully clear.										Sky perfectly clear, at 12h. 39m. a light breeze sprung up.			
57	39 0	485	376	861	938														
58	42 0	497	365	862	940														
59	45 0	488	390	878	957														
60	51 0	473	402	875	954														
61	54 0	447	420	867	945														
62	58 0	438	415	853	930														
	Mean...	477	392	869	945 ±1·1														
63	0 1 0	428	434	862	940	23 ·379	56 ·7	48 ·3											
64	4 0	432	431	863	941	Light breeze from S. Entire sky beauti-													
65	7 0	426	438	864	942														
66	10 0	428	434	862	940														
67	13 0	457	424	881	960														
68	16 0	456	415	871	949					362	0 16 0	347	509	841	917				
69	19 0	426	437	863	941					363	19 0	372	407	765	834				
70	22 0	411	430	841	917					364	22 0	426	360	772	841				
71	25 0	413	429	842	918					365	25 0	430	409	824	898				
72	28 0	414	449	863	941					366	28 0	387	377	750	818	27 ·620	75 ·5		
	Mean...	429	432	861	939 ±1 3						Mean...	392	·412	790	862 ±7·7				

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 0s) h. m. s.	1879, November 4.									(Slow 27s) h. m. s.	1879, November 4.							
73	11 32 0	520	354	874	953	23 ·414	53 ·0	44 ·9											
74	35 0	506	376	882	961														
75	38 0	504	390	894	974														
76	41 0	495	394	889	969														
77	44 0	490	394	884	964														
78	47 0	489	393	882	961														
79	50 0	489	397	886	966														
80	53 0	483	396	879	958														
81	56 0	479	400	879	958														
82	59 0	480	393	873	952	23 ·403	53 ·3	45 ·2											
	Mean...	494	389	883	962 ±0 ·6														
83	0 2 0	477	398	875	954														
84	5 0	478	396	874	953					367	0 4 0	332	481	798	870	27 ·689	73 ·8	..	132 ·8
85	8 0	476	396	872	950					368	7 0	388	423	796	868				
86	11 0	473	399	872	950					369	10 0	424	368	778	848				
87	14 0	471	409	880	959					370	13 0	430	379	794	865				
88	17 0	460	427	887	967					371	16 0	422	389	796	868				
89	21 0	453	419	872	950					372	19 0	424	386	795	867				
90	24 0	460	402	862	940					373	22 0	413	423	821	895				
91	27 0	461	409	870	948					374	25 0	395	408	789	860				
92	30 0	460	409	869	947					375	28 0	425	392	802	874	27 ·674	73 ·5	..	133 ·1
	Mean...	467	406	873	952 ±0 ·5						Mean...	406	405	797	868 ±1 ·1				

Remarks at Mussooree (printed vertically across the columns):

(Nos. 73–82) instrument from walling of a tent. En- slight gusts.

(Nos. 83–92) To-day and hereafter protected wind by means of the canvas Breeze from S., sometimes in tire sky beautifully clear.

Remark at Dehra (printed vertically across the columns, Nos. 367–375): A light breeze was blowing during the whole of the observa- tions, but the sky was perfectly clear.

AT MUSSOOREE.

Reference number.	Chronometer time. (Fast 13ˢ) h. m. s.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
		1879, November 6.							
93	11 32 0	501	387	888	968	23 ·446	55 ·1	46 ·8	107 ·4
94	36 0	496	395	891	971				
95	39 0	496	403	899	980				
96	42 0	506	383	889	969				
97	45 0	511	385	896	977				
98	48 0	510	378	888	968				
99	51 0	500	389	889	969				
100	54 0	495	386	881	960				
101	57 0	499	379	878	957				
102	0 0 0	496	378	874	953	23 ·438	55 ·3	47 ·2	109 ·2
	Mean...	501	386	887	967 ±0 ·8				
102	0 0 0	496	378	874	953				
103	3 0	480	401	881	960				
104	6 0	477	396	873	952				
105	9 0	477	392	869	947				
106	12 0	465	409	874	953				
107	15 0	466	399	865	943				
108	18 0	472	401	873	952				
109	21 0	484	395	879	958				
110	24 0	487	394	881	960				
111	27 0	478	400	878	957				
112	30 0	469	404	873	952				
	Mean...	477	397	874	953 ±0 ·4				

Light wind from S. Entire sky beautifully clear.

AT DEHRA.

Reference number.	Chronometer time. (Slow 22ˢ) h. m. s.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Wet.	Thermometer. In shade. Dry.	Thermometer. In sun, black bulb.
		1879, November 6.							
376	0 4 0	319	491	795	867	27 ·706	77 ·0	63 ·0	133 ·0
377	7 0	360	472	817	891				
378	10 0	357	466	808	881				
379	13 0	361	467	813	886				
380	16 0	416	405	806	879				
381	19 0	421	425	831	906				
382	22 0	406	430	821	895				
383	25 0	413	429	827	901				
384	28 0	398	431	814	887	27 ·691	77 ·2	63 ·0	134 ·0
	Mean...	383	446	815	888 ±1 ·0				

Sky perfectly clear; but the sun appeared as if shining through a thin haze extending some 2 or 3 degrees from its centre.

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 19s) h. m. s.	1879, November 7.							
113	11 32 0	237	620	857	934	23·415	54·1	44·0	108·0
114	36 0	296	562	858	935	S. wind brisk, but not felt behind usual canvas wall. Entire sky beautifully clear.			
115	39 0	330	553	883	962				
116	42 0	329	534	863	911				
117	45 0	340	533	873	952				
118	48 0	350	518	868	946				
119	51 0	353	518	871	949				
120	54 0	354	515	869	947				
121	57 0	361	508	869	947				
122	0 0 0	365	507	872	950	23·406	53·7	43·2	108·2
	Mean...	332	537	869	946 ±0·7				
122	0 0 0	365	507	872	950				
123	3 0	363	506	869	947				
124	6 0	360	497	857	934				
125	9 0	359	499	858	935				
126	12 0	362	497	859	936				
127	15 0	356	509	865	943				
128	19 0	338	530	868	946				
129	22 0	334	527	861	938				
130	25 0	358	513	871	949				
131	29 0	360	515	875	954				
	Mean...	356	510	866	943 ±0·7				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 21s) h. m. s.	1879, November 7.							
385	0 4 0	268	560	813	886	Sun shining through haze as yesterday, otherwise nothing to remark.			
386	7 0	268	541	794	865				
387	10 0	283	527	795	867				
388	13 0	293	508	787	858				
389	16 0	313	498	796	868				
390	19 0	328	479	792	863				
391	22 0	311	497	793	864				
392	25 0	283	503	772	841				
393	28 0	294	511	791	862				
	Mean...	293	514	793	864 ±0·9				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 26s)	1879, November 8.							
	h. m. s.								
132	11 32 0	437	396	833	908	23 ·471	55 ·7	47 ·1	105 ·2
133	35 0	436	393	829	904				
134	38 0	431	395	826	900				
135	41 0	430	396	826	900				
136	44 0	423	396	819	893				
137	47 0	419	408	827	901				
138	50 0	424	400	824	898				
139	53 0	420	406	826	900				
140	56 0	416	406	822	896				
141	59 0	409	408	817	891	23 ·465	56 ·4	46 ·7	107 ·4
	Mean...	425	400	825	899 ±0·4				
142	0 2 0	409	421	830	905				
143	5 0	415	410	825	899				
144	8 0	422	408	830	905				
145	11 0	418	414	832	907				
146	14 0	405	426	831	906				
147	17 0	407	419	826	900				
148	20 0	417	406	823	897				
149	23 0	416	419	835	910				
150	26 0	410	417	827	901				
151	29 0	410	416	826	900	23 ·462	55 ·0	46 ·2	108 ·0
	Mean...	413	416	829	903 ±0·4				

Air still. Some light strati were scattered all about up to 9 A.M. During observations a few cumuli to N. and W. near horizon, and a belt of strati full 40° below sun to south; otherwise sky quite clear.

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 12s) h. m. s.	1879, November 10.							
394	11 43 0	359	475	819	893	ing through haze as before, fectly clear. 10 A.M. clouds over the hills. 0h. 1m. P.M.	near sun. The afternoon observations could not be		
395	46 0	373	453	811	884				
396	49 0	369	486	840	916				
397	52 0	382	406	774	844				
398	55 0	410	372	768	837				
399	58 0	395	441	821	895				
	Mean...	381	439	806	878 ±4·7				
400	0 1 0	412	401	798	870	27·804	74·0	61·5	130·0
401	4 0	433	373	791	862	Morning, sun shin- otherwise sky per showing to north	light clouds cloudy, and further made.		
402	7 0	433	386	804	876				
403	10 0	465	353	803	875				
404	13 0	488	343	816	889				
405	16 0	478	321	785	856				
406	19 0	444	342	772	841				
407	22 0	447	341	774	844				
408	25 0	476	336	797	869				
409	28 0	451	365	801	873				
	Mean...	453	356	794	866 ±1·2				

AT MUSSOORIE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Slow 7s) h. m. s.	1879, November 12.									(Slow 1s) h. m. s.	1879, November 12.							
152	7 54 0	76	585	661	720					410	7 54 0	281	279	550	600				
153	57 0	132	536	668	728					411	57 0	311	266	567	618				
154	8 0 0	194	494	688	750					412	8 0 0	326	258	573	625				
155	3 0	231	464	695	758					413	3 0	332	253	574	626				
156	6 0	250	451	701	764					414	6 0	328	269	586	639				
	Mean...	177	506	683	744 ±3·3						Mean...	316	265	570	622 ±2·1				
157	8 54 0	335	472	807	880					415	8 54 0	345	356	688	750				
158	57 0	361	449	810	883					416	57 0	381	328	696	759				
159	9 0 0	367	429	796	868					417	9 0 0	414	301	702	765				
160	3 0	389	402	791	862					418	3 0	428	300	715	779				
161	6 0	399	405	798	870					419	6 0	449	269	705	768				
	Mean...	369	431	800	873 ±1·5						Mean...	403	311	701	764 ±1·6				
162	9 54 0	523	311	834	909					420	9 54 0	261	483	731	797				
163	57 0	522	305	827	901					421	57 0	264	511	761	829				
164	10 0 0	509	327	836	911	23·552	51·4	44·9		422	10 0 0	276	494	756	824				
165	3 0	506	329	835	910					423	3 0	306	456	748	815				
166	6 0	505	325	830	905					424	6 0	325	439	750	818				
	Mean...	513	319	832	907 ±0·7						Mean...	286	477	749	817 ±1·8				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	×	⊙ + × In terms of A. Glass on	Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Wet.	In sun, black bulb.
	(Slow 7s) h. m. s.	1879, November 12.							
167	10 54 0	317	527	844	920				
168	57 0	336	516	852	929				
169	11 0 0	361	478	839	915				
170	3 0	374	480	854	931				
171	6 0	374	477	851	928				
	Mean...	352	496	848	925 ±1·2				
172	11 31 0	378	481	859	936	On 9th, 10th, and 11th more or less clouds, but nights clear. To-day wind S., generally gentle, but sometimes in gusts. For a time a few (3 or 4) small light patches of cirro-cumuli floating about, and getting dissipated very oddly about the same spot and when some 30° to 40° from sun: otherwise sky quite clear.			
173	34 0	387	455	842	918				
174	37 0	399	466	865	943				
175	40 0	403	470	873	952				
176	43 0	387	467	854	931				
177	46 0	381	469	850	927				
178	49 0	383	469	852	929				
179	52 0	379	478	857	934				
180	55 0	384	477	861	938				
181	58 0	390	472	862	940				
	Mean...	387	470	857	935 ±0·7				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	×	⊙ + × In terms of A. Glass on	Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Wet.	In sun, black bulb.
	(Slow 1s) h. m. s.	1879, November 12.							
425	10 54 0	344	431	761	829				
426	57 0	367	433	786	857				
427	11 0 0	351	442	779	849				
428	3 0	343	453	782	852				
429	6 0	356	456	797	869				
	Mean...	352	443	781	851 ±2·0				
430	11 48 0	273	532	791	862	Sky clear. Sun shining through haze.			
431	51 0	315	480	781	851				
432	54 0	366	427	779	849				
433	57 0	376	432	793	864				
434	0 0 0	367	428	781	851				
	Mean...	339	460	785	855 ±1·3				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 7s) h. m. s.	1879, November 12.							
182	0 1 0	370	488	858	935	23 ·540	52 ·8	45 ·2	103 ·1
183	4 0	354	497	851	928				
184	7 0	357	492	849	925				
185	10 0	359	491	850	927				
186	13 0	355	508	863	941				
187	16 0	350	501	851	928				
188	19 0	356	493	849	925				
189	22 0	367	488	855	932				
190	25 0	369	498	867	945				
191	28 0	364	488	852	929				
	Mean...	360	494	854	932 ±0 ·7				
192	0 54 0	425	419	844	920				
193	57 0	399	435	834	909				
194	1 0 0	392	448	840	916				
195	3 0	394	442	836	911				
196	6 0	406	437	843	919				
	Mean...	403	436	839	915 ±0 ·8				

On 9th, 10th, and 11th more or less clouds, but nights clear. To-day wind S., generally gentle, but sometimes in gusts. For a time, a few (3 or 4) small light patches of cirro-cumuli floating about and getting dissipated very oddly about the same spot, and when some 30° to 40° from sun: otherwise sky quite clear.

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 1s) h. m. s.	1879, November 12.							
434	0 0 0	367	428	781	851				
435	3 0	359	447	791	862	27 ·853	74 ·5	61 ·5	132 ·0
436	6 0	346	434	766	835	Sky clear, sun shining through haze.			
437	9 0	342	423	751	819				
438	12 0	360	437	783	853				
	Mean...	355	434	774	844 ±2 ·8				
439	0 54 0	251	522	759	827				
440	57 0	300	500	786	857				
441	1 0 0	334	445	765	834				
442	3 0	345	444	775	845				
443	6 0	335	446	767	836	27 ·827	76 ·5	62 ·0	129 ·5
	Mean...	313	471	770	840 ±1 ·9				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In Shade. Dry.	Thermometer. In Shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 7s) h. m. s.	1879, November 12.							
197	1 54 0	289	517	806	879				
198	57 0	312	481	793	864				
199	2 0 0	347	457	804	876				
200	3 0	376	430	806	879				
201	6 0	383	425	808	881				
	Mean...	341	462	803	876 ±1·6				
202	2 54 0	407	350	757	825				
203	57 0	411	358	769	838				
204	3 0 0	399	366	765	834				
205	3 0	402	362	764	833				
206	6 0	393	370	763	832				
	Mean...	402	361	763	832 ±1·0				
207	3 54 0	242	439	681	742				
208	57 0	245	427	672	732				
209	4 0 0	248	426	674	735	23·507	51·0	43·6	
210	3 0	245	414	659	718				
211	6 0	243	407	650	709				
	Mean...	245	423	668	727 ±2·3				

On 9th, 10th, and 11th more or less clouds, but nights clear. To-day wind S., generally gentle, but sometimes in gusts. For a time, a few (3 or 4) small light patches of cirro-cumuli floating about and getting dissipated very oddly about the same spot, and when some 30° to 40° from sun: otherwise sky quite clear.

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 1s) h. m. s.	1879, November 12.							
444	1 54 0	502	268	756	824				
445	57 0	486	296	768	837				
446	2 0 0	472	320	778	848				
447	3 0	454	351	791	862	27·809	75·0	61·8	124·0
448	6 0	435	372	792	863				
	Mean...	470	321	777	847 ±2·7				
449	2 54 0	414	313	714	778				
450	57 0	423	304	714	778				
451	3 0 0	434	303	724	789				
452	3 0	407	315	709	773	27·795	75·0	60·7	88·5
453	6 0	343	346	677	738				
	Mean...	404	316	708	771 ±4·2				
454	3 54 0	262	297	549	598				
455	57 0	240	336	566	617				
456	4 0 0	222	350	562	613				
457	3 0	213	336	539	588				
458	6 0	215	313	518	565				
	Mean...	230	326	547	596 ±3·3				

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass on		Dry.	Wet.	
	(Fast 3ˢ) h. m. s.	1879, November 13.									(Fast 5ˢ) h. m. s.	1879, November 13.							
212	11 32 0	568	311	879	958	23·590	55·9	45·9	104·5	459	11 33 0	343	526	853	930	27·839	73·7	58·0	134·3
213	35 0	518	329	877	956	sky beautifully				460	36 0	369	416	800	872	Light gusts of			
214	38 0	530	346	876	955					461	39 0	395	446	826	900				
215	41 0	510	364	874	953					462	42 0	431	403	819	893				
216	44 0	510	378	888	968					463	45 0	426	400	811	884				
217	47 0	510	383	893	973					464	48 0	366	499	850	927				
218	51 0	520	343	863	941					465	51 0	345	478	809	882				
219	54 0	518	359	877	956					466	54 0	357	437	780	850				
220	57 0	506	382	888	968					467	57 0	373	419	778	848				
221	0 0 0	510	389	899	980	23·542	57·4	46·6	110·0	468	0 0 0	352	452	790	861	27·832	75·9	59·6	135·2
	Mean...	523	358	881	961 ±1·0	Light wind from S. Entire clear, brilliant weather.					Mean...	376	451	812	885 ±2·3	Less haze over sun to-day. wind at times, sky clear.			
221	0 0 0	510	389	899	980					468	0 0 0	352	452	790	861				
222	3 0	501	374	875	954					469	3 0	337	524	846	922				
223	6 0	491	388	879	958					470	6 0	367	451	803	875				
224	9 0	481	400	881	960					471	9 0	372	488	845	921				
225	12 0	476	404	880	959					472	12 0	380	475	840	916				
226	15 0	479	409	888	968					473	15 0	366	450	801	873				
227	18 0	483	406	889	969					474	18 0	334	490	809	882				
228	21 0	477	399	876	955					475	21 0	369	470	824	898				
229	24 0	469	400	869	947					476	24 0	418	428	831	906				
230	27 0	458	420	878	957					477	27 0	435	377	797	869				
231	30 0	450	420	870	948	23·537	56·6	48·2	110·9	478	30 0	442	420	846	922	27·822	74·9	58·6	132·5
	Mean...	480	401	881	960 ±0·8						Mean...	379	457	821	895 ±1·9				

AT MUSSOOREE.										AT DEHRA.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 15s) h. m. s.	1879, November 14.									(Fast 13s) h. m. s.	1879, November 14.							
232	7 55 0	145	555	700	763					479	7 56 0	178	405	573	625				
233	58 0	191	523	714	778					480	59 0	205	381	575	627				
234	8 1 0	238	491	729	795	23·537				481	8 2 0	232	373	594	647				
235	4 0	275	450	725	790					482	5 0	268	334	591	644				
236	7 0	305	427	732	798					483	8 0	313	294	596	650				
	Mean...	231	489	720	785 ±2·4						Mean...	239	357	586	639 ±2·1				
237	8 55 0	445	357	802	874					484	8 56 0	153	541	682	743				
238	58 0	454	361	815	888					485	59 0	199	519	705	768				
239	9 1 0	459	354	813	886					486	9 2 0	236	487	710	774				
240	4 0	463	354	817	891					487	5 0	248	460	695	758				
241	7 0	469	376	825	899					488	8 0	259	474	720	785				
	Mean...	458	356	814	888 ±1·5						Mean...	219	496	702	766 ±2·5				
242	9 55 0	532	326	858	935					489	9 56 0	420	359	765	834				
243	58 0	531	338	869	947					490	59 0	419	340	745	812				
244	10 1 0	530	332	862	940	23·560	54·8	43·2	81·6	491	10 2 0	417	396	798	870				
245	4 0	531	332	863	941					492	5 0	402	404	791	862				
216	7 0	533	330	863	941					493	8 0	409	399	793	864				
	Mean...	531	332	863	941 ±1·0						Mean...	413	380	778	848 ±4·2				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. $\odot$	Observed results reduced to 32° F. $\times$	$\odot + \times$ In terms of A. Glass on	$\odot + \times$ In terms of A. Glass on	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 15^s) h. m. s.	1879, November 14.							
247	10 55 0	455	428	883	962	South wind, but hardly perceptible. Entire sky most beautifully clear: weather quite brilliant.			
248	0 58 0	464	418	882	961				
249	11 1 0	470	416	886	966				
250	0 4 0	486	416	902	983				
251	0 7 0	477	403	880	959				
	Mean...	470	416	886	966 ±2·2				
252	11 32 0	507	401	908	990				
253	0 35 0	490	415	905	986				
254	0 38 0	477	411	888	968				
255	0 41 0	480	414	894	974				
256	0 44 0	483	408	891	971				
257	47 0	481	411	892	972				
258	50 0	488	410	898	979				
259	53 0	493	415	908	990				
260	56 0	498	405	903	984				
261	59 0	507	407	914	996	23·541	55·0	44·6	112·8
	Mean...	490	410	900	981 ±0·8				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. $\odot$	Observed results reduced to 32° F. $\times$	$\odot + \times$ In terms of A. Glass on	$\odot + \times$ In terms of A. Glass on	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	Fast 13^s) h. m. s.	1879, November 14.							
494	10 56 0	459	364	808	881				
495	59 0	456	370	811	884	27·861	71·5	56·7	133·2
496	11 2 0	454	381	820	894				
497	5 0	452	366	803	875				
498	8 0	429	395	809	882	Sky clear.			
	Mean...	450	375	810	883 ±1·0				
						Slight haze over sun.			
499	11 56 0	347	481	813	886				
500	59 0	373	455	813	886	27·831	72·3	56·8	135·6
	Mean...	360	468	813	886 ±0				

At Mussooree.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 15s) h. m. s.	1879, November 14.							
262	0 2 0	506	405	911	993	South wind, but hardly perceptible. Entire sky most beautifully clear: weather quite brilliant.			
263	5 0	499	400	899	980				
264	8 0	491	395	886	966				
265	11 0	488	407	895	976				
266	14 0	487	402	889	969				
267	17 0	486	404	890	970				
268	20 0	489	398	887	967				
269	23 0	490	403	893	973				
270	26 0	486	404	890	970				
271	29 0	485	408	893	973				
	Mean...	491	403	894	974 ±0·7				
272	0 55 0	460	400	860	937				
273	58 0	462	413	875	954				
274	1 1 0	461	420	881	960				
275	4 0	454	415	869	947				
276	7 0	450	409	859	936				
	Mean...	457	411	868	947 ±2·1				

At Dehra.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 13s) h. m. s.	1879, November 14.							
501	0 2 0	400	437	822	896				
502	5 0	414	418	817	891				
503	8 0	428	418	831	906				
	Mean...	414	424	823	898 ±2·1				
504	0 56 0	399	431	815	888				
505	59 0	403	451	839	915	27·803	74·5	57·5	135·6
506	1 2 0	417	416	818	892				
507	5 0	432	399	816	889				
508	8 0	423	428	836	911				
	Mean...	415	425	825	899 ±2·3				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 15s) h. m. s.	1879, November 14.							
277	1 55 0	479	373	852	929				
278	58 0	482	383	865	913				
279	2 1 0	483	385	868	946	23 ·509	53 ·6	44·2	
280	4 0	475	385	860	937				
281	7 0	474	386	860	937				
	Mean...	479	382	861	938 ±1·0				
282	2 55 0	525	288	813	886				
283	58 0	491	310	801	873				
284	3 1 0	478	321	799	871				
285	4 0	466	332	798	870				
286	7 0	446	348	794	865				
	Mean...	481	320	801	873 ±1·7				
287	3 55 0	275	442	717	782				
288	58 0	266	445	711	775				
289	4 1 0	260	446	706	770	23 ·498	51 ·7	43 ·2	
290	4 0	253	425	678	739				
291	7 0	260	423	683	744				
	Mean...	263	436	699	762 ±3·4				

S. wind, but hardly perceptible. Entire sky most beautifully clear: weather quite brilliant.

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Fast 13s) h. m. s.	1879, November 14.							
509	1 56 0	441	343	770	839				
510	59 0	468	326	780	850	27 ·785	74 ·5	58 ·3	132 ·6
511	2 2 0	481	322	791	862				
512	5 0	483	300	769	838				
513	8 0	440	360	786	857				
	Mean...	463	330	779	849 ±1 ·8				
514	2 56 0	463	279	729	795				
515	59 0	432	300	719	784	27 ·781	75 ·0	59 ·5	126 ·5
516	3 2 0	419	306	712	776				
517	5 0	409	319	715	779				
518	8 0	370	344	701	764				
	Mean...	419	310	715	780 ±1 ·7				
519	3 56 0	277	342	608	663				
520	59 0	272	314	575	627	27 ·777	73 ·0	59 ·5	
521	4 2 0	247	341	577	629				
522	5 0	225	334	549	598				
523	8 0	233	314	537	585				
	Mean...	251	329	569	620 ±4 ·8				

At Mussooree.										At Dehra.									
Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.			Reference number.	Chronometer time.	Observed results reduced to 32° F.		⊙ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.								In shade.		In sun, black bulb.
		⊙	×	Glass on	Glass off		Dry.	Wet.				⊙	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 25s) h. m. s.	1879, November 15.																	
292	11 32 0	416	478	894	974	23 ·489	54 ·4	45 ·9	109 ·9										
293	35 0	411	483	894	974														
294	38 0	408	487	895	976														
295	41 0	402	491	893	973														
296	44 0	404	496	900	981														
297	47 0	409	490	899	980														
298	50 0	419	482	901	982														
299	53 0	425	476	901	982														
300	56 0	426	469	895	976														
301	59 0	430	468	898	979	23 ·478	54 ·5	46 ·0	110 ·7										
	Mean...	415	482	897	978 ±0·3														
302	0 2 0	431	467	898	979														
303	5 0	430	485	915	997														
304	8 0	432	472	904	985														
305	11 0	426	470	896	977														
306	14 0	415	479	894	974														
307	17 0	412	482	894	974														
308	20 0	411	482	893	973														
309	23 0	405	489	894	974														
310	26 0	395	493	888	968														
311	29 0	393	489	882	961	23 ·466	54 ·6	46 ·1	110 ·6										
	Mean...	415	481	896	976 ±0·7														

Moderate breeze from S. Sky quite clear during observation for full 80° around zenith, but up to half-hour before commencing small cirro-cumuli were scattered all about.

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 9s) h. m. s.	1879, November 17.							
312	11 31 0	415	456	871	949	23·526	51·9	44·2	105·5
313	34 0	410	469	879	958				
314	37 0	416	450	866	944				
315	40 0	424	443	867	945				
316	43 0	421	456	877	956				
317	46 0	413	473	886	966				
318	49 0	410	460	870	948				
319	52 0	413	452	865	943				
320	55 0	420	415	865	943				
321	58 0	426	437	863	941				
	Mean...	417	454	871	949 ±0·8				
322	0 1 0	432	431	863	941	23·519	51·9	44·1	107·4
323	4 0	420	450	870	948	Moderate S. wind. Sky quite clear, except-	ing, say, a dozen small patches of cirro - cumuli,	none of which appeared nearer	than 45° of sun.
324	7 0	409	458	867	945				
325	10 0	412	462	874	953				
326	13 0	415	461	876	955				
327	16 0	417	451	868	946				
328	19 0	424	454	878	957				
329	22 0	417	450	867	945				
330	25 0	416	456	872	950				
331	28 0	412	458	870	948	23·511	52·0	44·6	107·5
	Mean...	417	453	870	949 ±0·4				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 22s) h. m. s.	1879, November 17.							
524	11 34 0	380	452	817	891	27·848	72·5	57·2	132·3
525	37 0	394	431	810	883				
526	40 0	357	485	827	901				
527	55 0	342	496	823	897				
528	58 0	328	514	827	901				
	Mean...	360	476	821	895 ±1·3				
529	0 1 0	305	564	853	930	27·835	73·2	57·5	135·1
530	4 0	308	503	796	868	Slight haze over sun; sky clear. Observations in-	terrupted for a short time by the smoke of a fire to the south.		
531	7 0	331	469	786	857				
532	10 0	351	487	823	897				
533	13 0	344	485	814	887				
534	16 0	346	482	813	886				
535	19 0	360	466	811	884				
536	22 0	363	471	819	893				
537	25 0	401	426	812	885				
538	28 0	403	426	814	887	27·822	75·3	58·0	134·4
	Mean...	351	478	814	887 ±1·5				

AT MUSSOOREE.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Dry.	Thermometer. In sun, black bulb.
	(Fast 6s) h. m. s.	1879, November 18.							
332	11 32 0	578	322	900	981	29·500	52·4	45·2	107·0
333	35 0	565	332	897	978	speck of cloud to day.			
334	38 0	550	337	887	967				
335	41 0	544	353	897	978				
336	44 0	536	365	901	982				
337	47 0	523	370	893	973				
338	50 0	511	372	883	962				
339	53 0	509	873	882	961				
340	56 0	510	365	875	954				
341	59 0	508	374	882	961	23·491	52·9	45·2	109·0
	Mean...	533	356	889	970 ±0·9	Not a			
342	0 2 0	502	375	877	956	Air quite still. A most brilliant be seen.			
343	5 0	491	395	886	966				
344	8 0	474	402	876	955				
345	11 0	469	403	872	950				
346	14 0	471	405	876	955				
347	17 0	454	413	867	945				
348	20 0	447	412	859	936				
349	23 0	451	427	878	957				
350	26 0	442	438	880	959				
351	29 0	442	434	876	955	23·489	52·7	45·4	109·1
	Mean...	464	410	874	953 ±0·7				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F. ⊙	Observed results reduced to 32° F. ×	⊙ + × In terms of A. Glass on	⊙ + × In terms of A. Glass off	Barometer reduced to 32° F.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
	(Slow 11s) h. m. s.	1879, November 18.							
539	11 32 0	270	552	807	880	27·818	69·4	55·6	134·4
540	35 0	307	536	828	903	As usual, a about 12.			
541	38 0	303	515	803	875				
542	41 0	305	498	789	860				
543	44 0	337	505	827	901				
544	47 0	374	462	821	895				
545	50 0	380	462	827	901				
546	53 0	381	431	797	869				
547	56 0	406	432	823	897				
548	59 0	430	416	831	906	27·805	71·6	55·6	132·2
	Mean...	349	481	815	889 ±1·5	Sky clear. Haze over sun. light breeze			
549	0 2 0	387	443	815	888				
550	5 0	356	474	815	888				
551	8 0	385	443	813	886				
552	11 0	404	428	817	891				
553	14 0	383	423	791	862				
554	17 0	376	462	823	897				
555	20 0	371	472	828	903				
556	23 0	345	480	810	883				
557	26 0	357	446	789	860				
558	29 0	362	480	827	901	27·792	72·3	55·7	132·4
	Mean...	373	455	813	886 ±1·8				

AT MUSSOOREE

Reference number.	Chronometer time.	Observed results reduced to 32° F.		☉ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.
		☉	×	Glass on	Glass off		Dry.	Wet.	
	(Fast 21ˢ) h. m. s.	1879, November 19.							
352	11 47 0	406	458	864	942	S. wind rather fresh.	Entire sky beautifully clear.		
353	50 0	401	466	867	945				
354	53 0	413	450	863	941				
355	56 0	424	446	870	948				
356	59 0	428	442	870	948	23·448	52·4	44·5	109·0
	Mean...	414	452	866	945 ±0·7				
357	0 2 0	430	433	863	941				
358	5 0	431	446	877	956				
359	8 0	429	441	870	948				
360	11 0	427	444	871	949				
361	14 0	430	449	879	958				
	Mean...	429	443	872	950 ±1·1				

AT DEHRA.

Reference number.	Chronometer time.	Observed results reduced to 32° F.		☉ + × In terms of A.		Barometer reduced to 32° F.	Thermometer.		
							In shade.		In sun, black bulb.
		☉	×	Glass on	Glass on		Dry.	Wet.	
	(Fast 1ˢ) h. m. s.	1879, November 19.							
559	11 32 0	274	538	797	869	27·761	70·4	56·0	131·8
560	35 0	316	507	808	881	Sky clear. Haze over sun, as usual.			
561	38 0	364	419	798	870				
562	41 0	340	516	841	917				
563	44 0	309	507	801	873				
564	47 0	350	471	806	879				
565	50 0	405	418	808	881				
566	53 0	406	428	819	893				
567	56 0	356	503	844	920				
568	59 0	360	483	828	903	27·753	72·0	56·5	132·0
	Mean...	348	482	815	889 ±1·6				
569	0 2 0	416	404	805	877				
570	5 0	442	372	799	871				
571	8 0	428	415	828	903				
572	11 0	418	414	817	891				
573	14 0	428	405	818	892				
574	17 0	415	388	789	860				
575	20 0	413	415	813	886				
576	23 0	414	410	809	882				
577	26 0	412	409	806	879				
578	29 0	426	390	801	873	27·741	71·8	56·3	131·0
	Mean...	421	402	809	881 ±0·9				

Table VI.—Group-Means of Radiation Results observed in India simultaneously at Mussooree with Actinometer A, and at Dehra with Actinometer B, expressed in terms of A, glass off.

1879.	At Mussooree, between						At Dehra, between					
	h. m. 11 30 to 11 40	h. m. 11 40 to 11 48	h. m. 11 48 to 0 0	h. m. 0 0 to 0 12	h. m. 0 12 to 0 20	h. m. 0 20 to 0 30	h. m. 11 30 to 11 40	h. m. 11 40 to 11 48	h. m. 11 48 to 0 0	h. m. 0 0 to 0 12	h. m. 0 12 to 0 20	h. m. 0 20 to 0 30
October 31	994_3	992_3	969_4	965_5	972_3	956_2						
November 1	928_3	940_2	967_4	946_4	934_2	958_1						
" 2	979_3	965_3	987_4	981_5	978_3	966_2						
" 3	945_3	949_2	943_3	941_4	950_3	925_3	..	..	..	..	876_2	852_3
" 4	963_3	965_3	959_4	952_4	959_3	945_3	..	..	..	862_3	867_3	876_3
" 6	973_3	971_3	960_4	953_5	951_3	956_3	..	..	..	880_3	892_4	894_2
" 7	944_3	946_3	948_4	940_5	945_2	947_3	..	..	..	873_3	863_3	856_3
" 8	904_3	898_3	896_4	904_4	901_3	904_3						
" 10	..	..	..	..	..	..	..	898_3	859_8	871_4	862_3	862_3
" 12	932_3	937_3	935_4	929_4	931_3	935_3	..	862_1	854_4	842_4	853_1	
" 13	956_3	965_3	961_4	962_6	964_3	951_3	901_3	901_3	860_4	899_5	884_3	899_3
" 14	981_3	972_3	987_4	979_4	969_3	972_3	..	..	886_2	898_3		
" 15	975_3	978_3	980_4	985_4	974_3	968_3						
" 17	950_3	956_3	944_4	947_4	953_3	948_3	892_3	..	899_2	888_4	886_3	888_3
" 18	975_3	978_3	960_4	957_4	945_3	957_3	886_3	885_3	898_4	888_4	887_3	881_3
" 19	..	942_1	946_4	949_4	958_1		873_3	890_3	899_4	886_4	879_3	878_3
Means of only *complete* Daily Series	957_{42}	958_{40}	957_{55}	953_{61}	952_{40}	949_{38}	887_9	892_9	884_{12}	891_{13}	883_9	886_9

Table VII.—Daily Means of Radiation Results for the half-hours preceding and succeeding Noon, observed in India simultaneously at Mussooree with Actinometer A, and at Dehra with Actinometer B, and expressed in terms of A, glass off.

AT MUSSOOREE = M.

1879.	Sun's meridional zenith distance.	Mean radiation for half-hour: Before noon.	Mean radiation for half-hour: After noon.	Mean of before and after noon.	At noon: Barometer reduced to 32° F.	At noon: Thermometer, In shade, Dry.	At noon: Thermometer, In shade, Wet.	At noon: Thermometer, In sun, black bulb.	Aspect of sky.
Oct. 31..	44°·5	983_{10} ±1·5	965_{10} ±1·0	974	...	...	...	...	No. 2
Nov. 1...	44·8	948_{9} ±2·2	944_{7} ±1·7	946	23·430	57·9	50·6	...	No. 3
,, 2	45·1	978_{10} ±1·1	977_{10} ±1·0	978	·400	57·4	48·1	...	No. 2
,, 3	45·4	945_{8} ±1·1	939_{10} ±1·3	942	·379	56·7	48·3	...	No. 2
,, 4	45·8	962_{10} ±0·6	952_{10} ±0·5	957	·403	53·3	45·2	...	No. 2
,, 6	46·4	967_{10} ±0·8	953_{11} ±0·4	960	·438	55·3	47·2	109·2	No. 2
,, 7	46·7	946_{10} ±0·7	943_{10} ±0·7	945	·406	53·7	43·2	108·2	No. 2
,, 8	47·0	899_{10} ±3·4	903_{10} ±0·4	901	·465	56·4	46·7	107·4	Nos. 3, 4
	45·7	954	947	950					
,, 10	...	...	...	...	...	...	...	...	...
,, 12	48·1	935_{10} ±0 7	932_{10} ±0·7	934	·540	52·8	45·2	103·1	No. 3
,, 13	48·4	961_{10} ±1·0	960_{11} ±0·8	961	·542	57·4	46·6	110·0	No. 1
,, 14	48·6	981_{10} ±0·8	974_{10} ±0·7	978	·541	55·0	44·6	112·8	No. 1
,, 15	48·9	978_{10} ±0·3	976_{10} ±0·7	977	·478	54·5	46·0	110·7	No. 3
,, 17	49·4	949_{10} ±0·8	949_{10} ±0·4	949	·519	51·9	44·1	107·4	No. 3
,, 18	49·6	970_{10} ±0·9	953_{10} ±0·7	962	·491	52·9	45·2	109·0	No. 1
,, 19	49·9	945_{5} ±0·7	950_{6} ±1·1	948	·448	52·4	44·5	109·0	No. 2
	49·0	960	956	958					
Means of columns.		956	951	954	...	...	...	...	...

AT DEHRA = D. / M − D.

1879.	Sun's meridional zenith distance.	Mean radiation for half-hour: Before noon.	Mean radiation for half-hour: After noon.	Mean of before and after noon.	At noon: Barometer reduced to 32° F.	At noon: Thermometer, In shade, Dry.	At noon: Thermometer, In shade, Wet.	At noon: Thermometer, In sun, black bulb.	Aspect of sky.	M − D: Before noon.	M − D: After noon.	M − D: Mean.
Oct. 31..	°											
Nov. 1...												
,, 2												
,, 3	45·3	...	862_{5} ±7·7	862	...	...	...	...	No. 5	...	77	77
,, 4	45·6	...	868_{9} ±1·1	868	27·689	73·8	...	132·8	No. 6	...	84	84
,, 6	46·2	...	888_{9} ±1·0	888	·706	77·0	63·0	133·0	No. 5	...	65	65
,, 7	46·5	...	864_{9} ±0·9	864	...	...	...	...	No. 5	...	79	79
,, 8												
	45·9		871	871								
,, 10	47·4	878_{6} ±4·7	866_{10} ±1·2	872	·804	74·0	61·5	130·0	No. 7			
,, 12	48·0	855_{5} ±1·3	844_{7} ±2·8	850	·833	74·5	61·5	132·0	No. 5	80	88	84
,, 13	48·2	885_{10} ±2·3	895_{11} ±1·9	890	·832	75·9	59·6	135·2	No. 5	76	65	71
,, 14	48·5	886_{8} ±0·0	898_{8} ±2·1	892	·831	72·3	56·8	135·6	No. 6	95	76	86
,, 15												
,, 17	49·3	895_{8} ±1·3	887_{10} ±1·5	891	·835	73·2	57·5	135·1	No. 5	54	62	58
,, 18	49·5	889_{10} ±1·5	886_{10} ±1·3	888	·805	71·6	55·6	132·2	No. 5	81	67	74
,, 19	49·7	889_{10} ±1·6	881_{10} ±0·9	885	·753	72·0	56·5	132·0	No. 5	56	69	63
	48·7		880	881								
Means of columns.	...	882	876	877	...	...	...	...	...	74	73	74

Aspect of Sky.

No. 1 stands for sky beautifully clear; most brilliant weather.
No. 2 ,, sky beautifully clear.
No. 3 ,, sky clear for at least 30° to 40° around sun; elsewhere a few (perhaps under a dozen) small patches of light cirro-cumuli scattered about.
No. 4 stands for sky clear for at least 30° to 40° around sun; elsewhere belts of thin strati generally along horizon for some 5° to 10°.
No. 5 ,, sky perfectly clear, but thin haze over sun.
No. 6 ,, sky perfectly clear, but thin haze over sun. Light breeze.
No. 7 stands for clouds showing over hills to north, otherwise sky perfectly clear.

Table VIII.—Long Series; Mean Radiation Results for groups at successive hours, observed in India simultaneously at Mussooree with Actinometer A, and at Dehra with Actinometer B, and expressed in terms of A, glass off.

Appt. Time.	At Mussooree = M.						At Dehra = D.						M − D for Means of Radiation.
	Sun's Zenith Dist.	Solar Radiation.		Radiation Defect from Noon.			Sun's Zenith Dist.	Solar Radiation.		Radiation Defect from Noon.			
		Nov. 12.	Nov. 14.	Nov. 12.	Nov. 14.	Mean.		Nov. 12.	Nov. 14.	Nov. 12.	Nov. 14.	Mean.	
h. m.	°						°						
8 0	75 ·3	744_5	785_5	190	193	192	75 ·1	622_5	639_5	228	253	241	134
9 0	64 ·9	873_5	888_5	61	90	76	64 ·8	764_5	766_5	86	126	106	116
10 0	56 ·3	907_5	941_5	27	37	32	56 ·2	817_5	848_5	33	44	39	91
11 0	50 ·5	925_5	966_5	9	12	11	50 ·3	851_5	883_5	1	9	4	79
	Sum before Noon ..			287	332	311	..	..	..	346	432	390	
0 0	48 ·4	934_{20}	978_{20}	..	..	..	48 ·2	850_{10}	892_5				85
1 0	50 ·5	915_5	947_5	19	31	25	50 ·3	840_5	899_5	10	− 7	2	61
2 0	56 ·4	876_5	938_5	58	40	49	56 ·2	847_5	849_5	3	43	23	59
3 0	64 ·9	832_5	873_5	102	105	104	64 ·8	771_5	780_5	79	112	96	77
4 0	75 ·3	727_5	762_5	207	216	212	75 ·2	596_5	620_5	254	272	263	137
	Sum after Noon ..			386	392	390	..	..	..	346	420	384	
	After Noon − Before Noon ..			99	60	79	..	..	..	0	−12	−6	

P 2

Table VII.—Daily Means of Radiation Results for the half-hours preceding and succeeding Noon, observed in India simultaneously at Mussooree with Actinometer A, and at Dehra with Actinometer B, and expressed in terms of A, glass off.

1879.	AT MUSSOOREE = M.									AT DEHRA = D.									M − D.		
	Sun's meridional zenith distance.	Mean radiation for half-hour		Mean of before and after noon.	At noon.				Aspect of sky.	Sun's meridional zenith distance.	Mean radiation for half-hour		Mean of before and after noon.	At noon.				Aspect of sky.	Before noon.	After noon.	Mean.
					Barometer reduced to 32° F.	Thermometer.								Barometer reduced to 32° F.	Thermometer.						
						In shade.		In sun, black bulb.							In shade.		In sun, black bulb.				
		Before noon.	After noon.			Dry.	Wet.				Before noon.	After noon.			Dry.	Wet.					
	°									°											
Oct. 31..	44·5	$983_{10} \pm 1·5$	$965_{10} \pm 1·0$	974	...	...	...	...	No. 2												
Nov. 1...	44·8	$948_{9} \pm 2·2$	$944_{7} \pm 1·7$	946	23·430	57·9	50·6	...	No. 3												
„ 2	45·1	$978_{10} \pm 1·1$	$977_{10} \pm 1·0$	978	·400	57·4	48·1	...	No. 2												
„ 3	45·4	$945_{8} \pm 1·1$	$939_{10} \pm 1·3$	942	·379	56·7	48·3	...	No. 2	45·3	...	$862_{5} \pm 7·7$	862	...	...	...	...	No. 5	...	77	77
„ 4	45·8	$962_{10} \pm 0·6$	$952_{10} \pm 0·5$	957	·403	53·3	45·2	...	No. 2	45·6	...	$868_{9} \pm 1·1$	868	27·689	73·8	...	132·8	No. 6	...	84	84
„ 6	46·4	$967_{10} \pm 0·8$	$953_{11} \pm 0·4$	960	·438	55·3	47·2	109·2	No. 2	46·2	...	$888_{9} \pm 1·0$	888	·706	77·0	63·0	133·0	No. 5	...	65	65
„ 7	46·7	$946_{10} \pm 0·7$	$943_{10} \pm 0·7$	945	·406	53·7	43·2	108·2	No. 2	46·5	...	$864_{9} \pm 0·9$	864	...	...	...	...	No. 5	...	79	79
„ 8	47·0	$899_{10} \pm 3·4$	$903_{10} \pm 0·4$	901	·465	56·4	46·7	107·4	Nos. 3, 4												
	45·7	954	947	950						45·9		871	871								
„ 10	...	...	...	...	...	...	...	...	...	47·4	$878_{8} \pm 4·7$	$866_{10} \pm 1·2$	872	·804	74·0	61·5	130·0	No. 7			
„ 12	48·1	$935_{10} \pm 0·7$	$932_{10} \pm 0·7$	934	·540	52·8	45·2	103·1	No. 3	48·0	$855_{6} \pm 1·3$	$844_{5} \pm 2·8$	850	·853	74·5	61·5	132·0	No. 5	80	88	84
„ 13	48·4	$961_{10} \pm 1·0$	$960_{11} \pm 0·8$	961	·542	57·4	46·6	110·0	No. 1	48·2	$885_{10} \pm 2·3$	$895_{11} \pm 1·9$	890	·832	75·9	59·6	135·2	No. 5	76	65	71
„ 14	48·6	$981_{10} \pm 0·8$	$974_{10} \pm 0·7$	978	·541	55·0	44·6	112·8	No. 1	48·5	$886_{8} \pm 0·0$	$898_{8} \pm 2·1$	892	·831	72·3	56·8	135·6	No. 6	95	76	86
„ 15	48·9	$978_{10} \pm 0·3$	$976_{10} \pm 0·7$	977	·478	54·5	46·0	110·7	No. 3												
„ 17	49·4	$949_{10} \pm 0·8$	$949_{10} \pm 0·4$	949	·519	51·9	44·1	107·4	No. 3	49·3	$895_{5} \pm 1·3$	$887_{10} \pm 1·5$	891	·835	73·2	57·5	135·1	No. 5	54	62	58
„ 18	49·6	$970_{10} \pm 0·9$	$953_{10} \pm 0·7$	962	·491	52·9	45·2	109·0	No. 1	49·5	$889_{10} \pm 1·5$	$886_{10} \pm 1·3$	888	·805	71·6	55·6	132·2	No. 5	81	67	74
„ 19	49·9	$945_{5} \pm 0·7$	$950_{6} \pm 1·1$	948	·448	52·4	44·5	109·0	No. 2	49·7	$889_{10} \pm 1·6$	$881_{10} \pm 0·9$	885	·753	72·0	56·5	132·0	No. 5	56	69	63
	49·0	960	956	958						48·7		880	881								
Means of columns.		956	951	954	...	...	...	...	...	...	882	876	877	...	...	...	...	...	74	73	74

Aspect of Sky.

No. 1 stands for sky beautifully clear; most brilliant weather.
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No. 3 „ sky clear for at least 30° to 40° around sun; elsewhere a few (perhaps under a dozen) small patches of light cirro cumuli scattered about.
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No. 7 stands for clouds showing over hills to north, otherwise sky perfectly clear.

Table VIII.—Long Series; Mean Radiation Results for groups at successive hours, observed in India simultaneously at Mussooree with Actinometer A, and at Dehra with Actinometer B, and expressed in terms of A, glass off.

Appt. Time.	At Mussooree = M. Sun's Zenith Dist.	Solar Radiation. Nov. 12.	Solar Radiation. Nov. 14.	Radiation Defect from Noon. Nov. 12.	Radiation Defect from Noon. Nov. 14.	Radiation Defect from Noon. Mean.	At Dehra = D. Sun's Zenith Dist.	Solar Radiation. Nov. 12.	Solar Radiation. Nov. 14.	Radiation Defect from Noon. Nov. 12.	Radiation Defect from Noon. Nov. 14.	Radiation Defect from Noon. Mean.	M − D for Means of Radiation.
h. m.	°						°						
8 0	75 ·3	744_5	785_5	190	193	192	75 ·1	622_5	639_5	228	253	241	134
9 0	64 ·9	873_5	888_5	61	90	76	64 ·8	764_5	766_5	86	126	106	116
10 0	56 ·3	907_5	941_5	27	37	32	56 ·2	817_5	848_5	33	44	39	91
11 0	50 ·5	925_5	966_5	9	12	11	50 ·3	851_5	883_5	1	9	4	79
	Sum before Noon ..			287	332	311	..	..	..	346	432	390	
0 0	48 ·4	934_{20}	978_{20}	..	..	..	48 ·2	850_{10}	892_5				85
1 0	50 ·5	915_5	947_5	19	31	25	50 ·3	840_5	899_5	10	− 7	2	61
2 0	56 ·4	876_5	938_5	58	40	49	56 ·2	847_5	849_5	3	43	23	59
3 0	64 ·9	832_5	873_5	102	105	104	64 ·8	771_5	780_5	79	112	96	77
4 0	75 ·3	727_5	762_5	207	216	212	75 ·2	596_5	620_5	254	272	263	137
	Sum after Noon ..			386	392	390	..	..	..	346	420	384	
	After Noon − Before Noon ..			99	60	79	..	..	..	0	−12	−6	

Table IX.—Simultaneous experimental Observations at Dehra, the two Actinometers A and B being set side by side, and read by means of the same Chronometer.

1880. February 27. Apparent Time.		Observed results reduced to 32° F.				Radiation by		dA.	dB.	$dA-dB$.	Values exceeding 10.	
Beginning.	End.	A ⊙	A ×	B. ⊙	B. ×	A.	B.				dA.	dB.
h. m. s.	h. m. s.											
11 39 0	11 40 0	425	..	405								
40 30	41 30	..	416	..	439	832	846	+17	+ 6	+11	+17	+ 6
42 0	43 0	407	..	408								
43 30	44 30	..	413	..	445	817	853	+ 2	+13	−11	+ 2	+13
45 0	46 0	400	..	407								
46 30	47 30	..	437	..	473	833	870	+18	+30	−12	+18	+30
48 0	49 0	391	..	386								
49 30	50 30	..	433	..	455	832	851	+17	+11	+ 6	+17	+11
51 0	52 0	407	..	405								
52 30	53 30	..	430	..	437	829	842	+14	+ 2	+12	+14	+ 2
54 0	55 0	390	..	404								
55 30	56 30	..	398	..	410	798	833	−17	− 7	−10	−17	− 7
57 0	58 0	409	..	441								
58 30	59 30	..	409	..	404	818	839	+ 3	− 1	+ 4		
12 0 0	12 1 0	408	..	429								
1 30	2 30	..	404	..	415	805	834	−10	− 6	− 4	−10	− 6
3 0	4 0	393	..	409								
4 30	5 30	..	415	..	425	825	846	+10	+ 6	+ 4	+10	+ 6
6 0	7 0	427	..	433								
7 30	8 30	..	388	..	412	813	841	− 2	+ 1	− 3		
9 0	10 0	422	..	424								
10 30	11 30	..	404	..	410	812	835	− 3	− 5	+ 2		
12 0	13 0	393	..	425								
13 30	14 30	..	415	..	400	809	830	− 6	−10	+ 4	− 6	−10
15 0	16 0	395	..	435								
16 30	17 30	..	416	..	412	806	838	− 9	− 2	− 7		
18 0	19 0	384	..	417								
19 30	20 30	..	404	..	407	796	824	−19	−16	− 3	−19	−16
21 0	22 0	399	..	417								
22 30	23 30	..	433	..	434	816	843	+ 1	+ 3	− 2		
24 0	25 0	367	..	400								
25 30	26 30	..	422	..	408	809	824	− 6	−16	+10	− 6	−16
27 0	28 0	407	..	432								
28 30	29 30	..	389	..	396	800	825	−15	−15	0	−15	−15
30 0	31 0	414	..	425								
					Mean..	815	840			Sum..	151	138

Table X.—Observations with Actinometer A, by Captain J. P. Basevi, R.E.

1871.	Apparent time.	Zenith distance, meridional.	Radiation reduced to 32° F., and in terms of A, glass off.	Radiation defect from noon.	Barometer.	Thermometer. In shade. Dry.	Thermometer. In shade. Wet.	Thermometer. In sun, black bulb.
At Sialkot. Lat. 32° 31′. Long. 74° 36′. Height, 835 feet.								
	h. m.							
April 26 ..	11 41	19°·1	913_7	..	29·48	85·8	75·3	151·4
At Jalapur. Lat. 32° 33′. Long. 74° 16′. Height, 850 feet.								
April 28 ..	12 7	18·5	898_7	..	29·35	92·9	78·6	162·2
At Srinagar. Lat. 34° 5′. Long. 74° 51′. Height, 5,209 feet.								
May 15 ...	11 0	..	867_3	40	24·93	72·2	65·0	139·0
	12 0	15·3	907_3	..	·93	74·5	66·4	143·5
	1 0	..	904_3	3	·91	77·0	67·9	..
	2 0	..	925_3	−18	·91	78·9	68·5	..
	3 0	..	895_3	12	·88	80·0	69·7	..
	4 0	..	860_3	47	87	80·0	69·7	..
May 16 ...	8 0	..	772_3	137	24·93	63·5	59·6	111
	9 0	..	822_3	87	..	..	..	125
	10 0	..	871_3	38	·95	71·2	64·0	134·8
	11 0	..	905_3	4	·96	74·8	66.8	142·8
	12 0	15·1	909_3	..	·92	77·0	68·1	145·0
	1 0	..	927_3	−18	·92	79·8	68·7	147·5
May 19 ...	8 0	..	782_7	137	24·81	63·8	60·2	110
	9 0	..	859_7	16	·83	66·2	61·4	125
	10 0	..	900_7	19	·85	68·4	62·5	133·8
	11 0	..	904_7	15	·82	72·0	62·5	139
	12 0	14·4	919_7	..	·83	74·0	64·4	141·7
	1 0	..	897_5	22	·81	75·6	64·5	143·2
	2 0	..	886_7	33	·80	77·4	65·0	143·7
	3 0	..	875_7	44	·80	78·5	63·6	137·0
	4 0	..	823_4	96	·76	79·0	64·8	133

II. "On the Critical Point." By W. Ramsay, Ph.D., Professor of Chemistry in University College, Bristol. Communicated by E. J. Mills, D.Sc., F.R.S., Young Professor of Technical Chemistry in Anderson's College, Glasgow. Received November 8, 1880.

The experiments to be described were undertaken with a view to determine the difference in behaviour of two pure compounds, and a mixture of the two, at high temperatures, and under great pressures.

The two liquids selected were benzene, C_6H_6, and ether, $C_4H_{10}O$; for both are remarkably stable bodies, and both can be obtained easily, and in a perfectly pure condition. They are also without action on each other.

The benzene was produced by the distillation of calcium benzoate with lime; it was dried by cohobation with sodium for four days, and then distilled. It boiled at 81°·4 at a pressure of 750 millims. The ether was also cohobated with sodium for four days, and boiled at 34°·4.

The pressure-apparatus resembled that used by Dr. Andrews in his experiments on the critical state of carbonic acid, somewhat modified to suit the altered conditions. The gauge for measuring pressure was a carefully calibrated thermometer tube with round bore. No correction of capacity was necessary to allow for a conical space at the end, as is usual with air manometers, for the sealing of the tube was accomplished by drawing up a plug of fusible metal, and allowing it to solidify.

As the gauge lay horizontally, no correction was necessary for varying height of mercury. It was carefully filled with dry air, after its capacity had been ascertained by filling it with mercury, and subsequently weighing the mercury. The probable error of the volume of one division is 0·23 per cent., and the probable error of the total capacity 0·084 per cent. The measurements of pressure may therefore be regarded as a close approximation to absolute correctness, and as perfectly correct relatively to each other.

The temperature was corrected for the mercury outside of the heating apparatus, and after the experiments were over, was compared with a good unused new thermometer. The readings did not differ by more than 0·5 degree.

The liquid to be examined was contained in a piece of very narrow barometer-tubing, graduated in millimetres, and was introduced at a temperature close to its boiling point, after a portion had been boiled off, so as to ensure absence of air. While entering the experimental tube the liquid never came in contact with air, and from the results of experiments it appears that none was present.

Observations of the condition of the liquid experimented on, and

readings of its volume, were taken by means of a telescope placed about one foot from the tube. The readings to tenths of divisions are computed by eye, and are only approximately correct.

The tube containing liquid was heated in a copper block, in which a slit was cut open at the upper surface of the block, to permit observations being taken. The block was covered with a plate of glass and heated. It was found possible to keep the temperature constant to within $\frac{1}{10}$th of a degree for several hours by this arrangement.

Benzene was selected for the first set of experiments. While temperature was kept constant, volume was altered, and the corresponding pressure noted. The volumes are given in divisions of the tube, for the bore was almost perfectly uniform, and the unavoidable error in reading more than compensated any correction which might have been made.

The numbers refer to the curves on the accompanying woodcuts; the curves are isothermal. The pressure corresponding to each change of volume forms the ordinate of the curve, and the volume itself the abscissa.

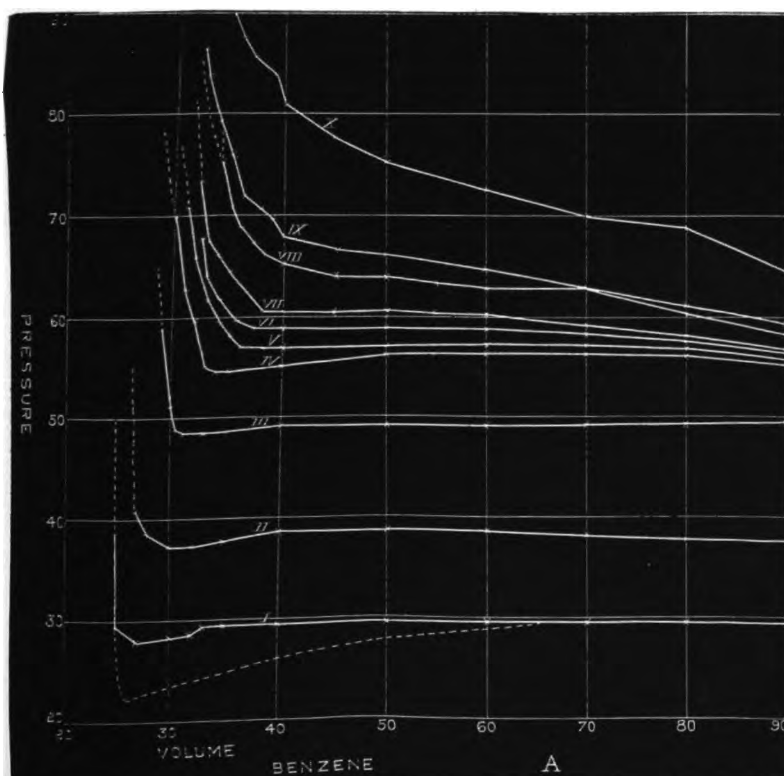


Table A.—Isotherms for Benzene.

Volume.	Pressure in Atmospheres.									
	I. T = 234°·7.	II. 252°·9.	III. 274°·4.	IV. 284°·1.	V. 286°·2.	VI. 288°·4.	VII. 291°·7.	VIII. 296°·0.	IX. 301°·4.	X. 312°·3.
90	29·37	37·88	49·21	55·29	55·83	56·49	56·60	58·21	59·54	64·80
80	29·56	37·88	49·21	56·27	56·82	57·63	58·09	60·30	61·05	68·85
70	29·52	38·19	49·21	56·38	57·17	58·45	59·30	63·08	63·06	69·86
60	29·37	38·72	49·21	56·38	57·05	58·93	60·30	63·08	64·94	72·69
55	..	..	..	..	..	..	60·30	63·50		
50	29·40	38·65	49·21	56·38	57·05	58·81	60·68	64·22	66·30	75·37
45	..	..	..	..	..	..	60·55	64·22	66·92	77·61
40	29·34	38·60	49·21	54·97	56·94	58·81	60·55	65·54	67·87	80·90
39	..	..	..	..	..	..	..	..	69·85	83·99
38	..	..	..	..	57·05	..	60·55	66·30	70·54	
37	..	..	..	..	..	58·81	..	67·23	..	86·01
36	..	..	..	..	57·17	..	..	68·85	71·96	87·44
35	29·34	37·73	48·71	54·76	58·33	‡59·67	64·22	70·70	76·17	90·26
34	..	..	..	54·66	59·67	61·98	65·99	75·87	79·10	
33	29·37	37·44	48·46	†54·97	61·72	64·22	67·44	..	83·72	
32	28·53	37·29	48·54	59·79	65·69	§67·71	73·25	..	‖86·52	
31	..	..	..	62·94	71·07					
30	28·24	37·77	*48·87 51·23 58·80	70·20						
29	..	..								
28	..	38·98								
27	28·05	40·86								
26										
25·1	29·20									
25	30·18									

* V = 30·5. † V = 33·2. ‡ V = 35·5. § V = 32·5. ‖ V = 32·5.

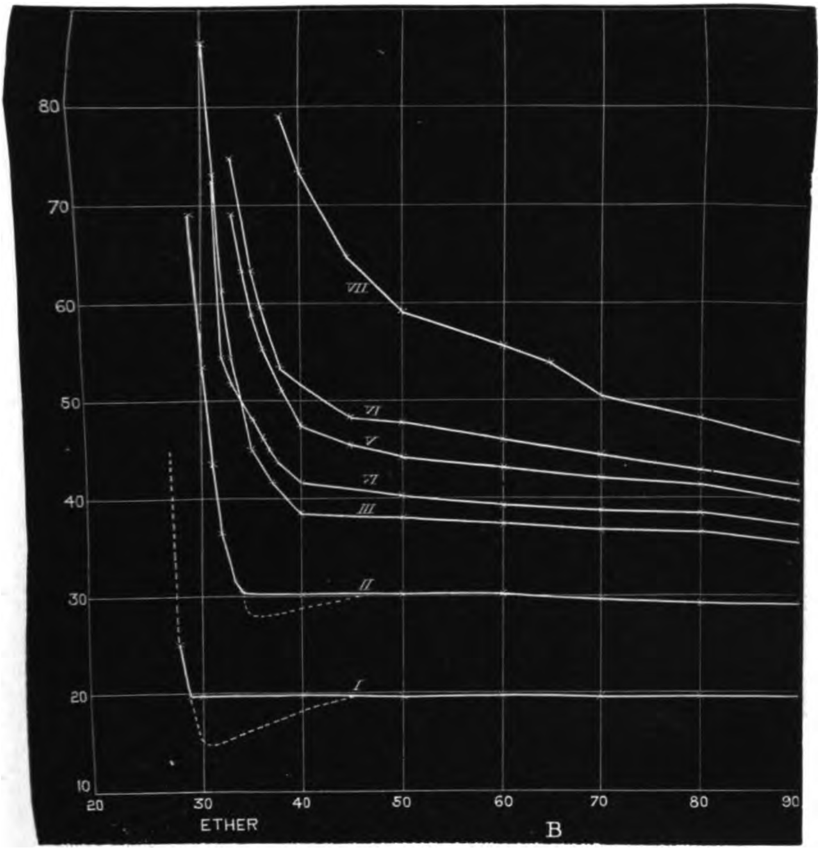


Table B.—Isotherms for Ether.

Volumes.	Pressure in Atmospheres.						
	I. T = 154°·7.	II. 179°·8.	III. 193°·5.	IV. 195°·5.	V. 201°·9.	VI. 207°·1.	VII. 228°.
90	19·52	29·07	35·53	37·44	39·72	41·45	45·75
80	19·60	29·47	36·53	38·55	41·45	42·94	48·07
70	19·67	29·71	36·76	38·86	42·12	44·40	50·33
65	..	..	..	..	..	..	53·73
60	19·83	30·12	37·39	39·50	43·07	45·97	55·61
50	19·69	30·12	38·08	40·17	44·38	47·81	59·05
45	..	..	..	..	45·39	48·30	64·80
40	19·88	30·12	38·39	41·45	48·30	51·51	73·25
38	..	..	..	43·59	50·78	53·33	78·88
37	..	..	41·45	44·68	..		
36	..	..	..	46·19	55·50	59·54	
35	19·69	30·12	45·03	47·97	58·93	63·22	
34	..	30·31	..	48·87	63·22		
33	..	31·61	54·76	51·79	68·84	74·78	
32	..	36·00	61·46	54·76			
31	..	43·55	73·06	72·50			
30	19·72	53·43	86·52				
29	..	68·85					
28	25·10						

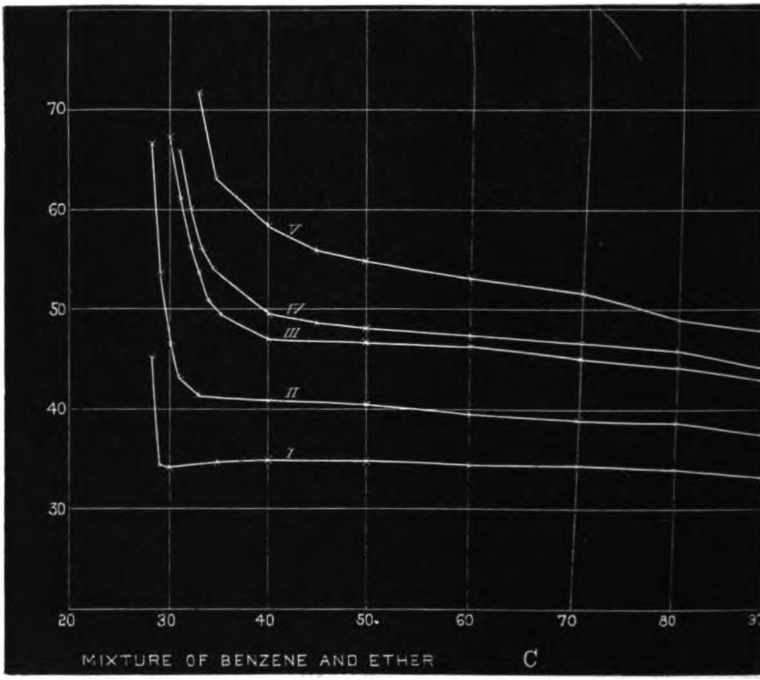


Table C.—Isotherms for a Mixture of Equal Weights of Benzene and Ether.

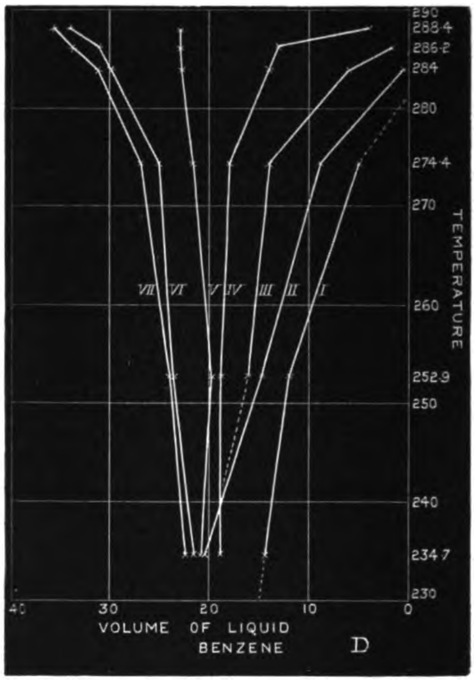
Volumes.	Pressure in Atmospheres.				
	I. T=215°·6.	II. 225°·2.	III. 235°·6.	IV. 240°·7.	V. 252°·9.
90	33·15	37·78	43·07	43·99	47·81
80	34·05	38·86	44·32	45·75	49·04
70	34·14	38·86	45·25	46·49	51·70
60	34·55	39·40	46·27	47·33	53·23
50	34·93	40·62	46·42	47·78	54·99
45	..	..	..	48·46	56·16
40	35·06	40·86	46·50	49·29	58·51
38	..	..	..	..	60·55
35	34·75	41·14	49·38	53·38	63·22
34	..	..	51·05	53·83	71·59
33	..	41·45	53·73	56·60	76·17
32	..	..	56·38	60·17	
31	..	43·33	61·33	65·84	
30	34·34	47·66	67·39		
29	34·55	53·93			
28	45·02	66·48			

It may be remembered that Dr. Andrews, in his "Research on Carbonic Anhydride," never obtained the gas absolutely free from air, consequently, in diminishing the volume of his gas, in contact with its liquid, he always noticed that a slight increase of pressure was necessary. If the curves representing the behaviour of benzene under similar circumstances be referred to, it will be noticed that the pressure actually is reduced, in producing diminution of volume, in Curves I, II, III, and IV. A relic of the same form of curve remains in the behaviour of a mixture of ether and benzene, but no trace is observable in the behaviour of ether. The explanation appears to be that the molecules, when the gas has been compressed to a certain extent (very shortly before all gas is condensed to liquid), begin to exert some attraction for each other, and consequently relieve the pressure. The explanation of the fact that this phenomenon is noticeable in the case of benzene, but not with ether, is perhaps connected with their different behaviour at higher temperatures: the meniscus of benzene is always easily distinguished, even up to its vanishing point; whereas that of ether soon becomes extremely mist-like and hazy. I have little doubt that many other substances, when heated under pressure in a condition absolutely free from the admixture of any other gas, will show similar results.

Probably, closely connected with this observation, is another:—namely, that it is possible, after condensing all gas to liquid by pressure, to lower the pressure very considerably without ebullition of the liquid, and consequently without formation of gas. At a temperature of 228° for instance, it is possible gradually to reduce the pressure from 29 to 22·4 atmospheres without any evolution of gas in the case of benzene; sudden ebullition then takes place, and the pressure rises to 29·3 atmospheres, the volume at the same time suddenly increasing to 65. This behaviour is represented by a dotted line in the diagram representing the isotherm for benzene. The dotted line in the second isotherm represents a similar phenomenon; the pressure could be reduced to 35·4 atmospheres before sudden ebullition took place. At higher temperatures, a very slight reduction of pressure caused ebullition, but the phenomenon could still be noticed, although no attempt to measure it was made. The same phenomena were noticed with ether, and are also exhibited on the diagram.

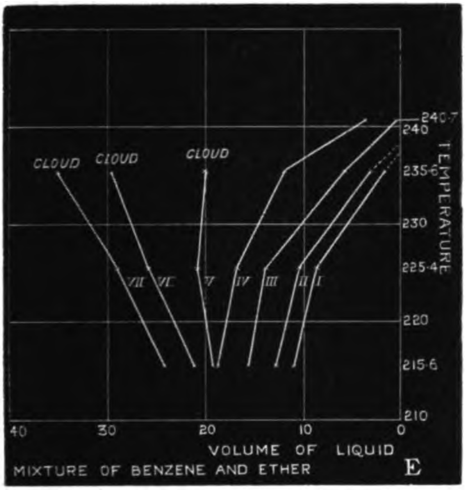
It is necessary, before discussing the results of these observations, to give some tables, showing the relative proportions of gas to liquid at the various temperatures chosen; always, of course such that it was possible to distinguish the two states of matter easily from each other. I have thought it sufficient to reproduce merely those of benzene and of its mixture with ether; for the behaviour of ether does not materially differ from that of the mixture. The curves constructed to exhibit these relations graphically have for their ordinates

the volumes of liquid capable of existing at particular temperatures, the latter being represented as abscissæ. The curves may be termed curves of *equivolume*, for the total volume of gas and liquid was maintained constant through each.



Benzene. Curve of Equivolume, expressing Proportion of Liquid at each Temperature.

Temp.....	234°·7	252°·9	274°·4	284°·1	286°·2	288°·4
Volume.						
90 ..	14·5 ..	11·9 ..	5·0 ..	0 ..	0 ..	0
80 ..	20·2 ..	14·7 ..	8·9 ..	0·2 ..	0 ..	0
70 ..	— ..	16·8 ..	14·0 ..	6·1 ..	1·5 ..	0
60 ..	19·1 ..	19·0 ..	17·8 ..	14·1 ..	13·0 ..	4
50 ..	20·5 ..	20·9 ..	21·5 ..	22·8 ..	23·0 ..	22·8
40 ..	21·3 ..	23·7 ..	25·2 ..	30·0 ..	30·8 ..	34·0
35 ..	22·1 ..	23·9 ..	27·0 ..	31·2 ..	32·5 ..	35·5



Mixture of Equal Weights of Benzene and Ether.

Temperature........ Volume.	215°·6	225°·2	235°·6	240°·7
90	11·0	8·7	1·6	0
80	13·0	10·2	3·0	0
70	15·7	13·8	5·9	0·2
60	18·8	16·9	13·0	3·4
50	19·0	20·9	20·0	cloud
40	21·0	25·9	29·8	,,
35	24·1	29·0	35·0	35·0

The critical point.—The critical temperature of benzene lies about 291°·7. At that temperature, no decided meniscus could be produced by slowly altering the volume of the substance, but the tube remained full of flickering striæ. On increasing volume from 50 to 70, the flickering striæ disappeared, and the matter contained in the tube appeared to be wholly converted into gas. On the other hand, by diminishing the volume to 35 divisions, the flickering appearance again vanished, for the whole of the contents of the tube were condensed to liquid. At higher temperature, no line of demarcation between liquid and gas was observable.

This observation, and similar ones made with ether and with a mixture of benzene, contradict the statement made by me in a note published in the "Proc. Roy. Soc.," vol. 30, p. 323, I stated there that the temperature at which the meniscus disappears depends on the relative volumes of the liquid and gas. I have now to acknowledge that the observations on which this statement were

based, lead to an opposite conclusion when correctly explained. The heat in these experiments was imparted to the tubes through a large block of copper, in grooves in which the tubes were placed. I have no doubt that the temperature of the copper block represented accurately that of the tubes; but *not that of the liquid contained in the tube.* When the temperature of the tube is raised, however slowly, evaporation of the liquid in the tube ensues, and time is required for evaporation. During this time, the temperature of the copper block is rising; and with a tube insufficiently filled, it appears necessary to allow a longer time for evaporation, than with one containing more liquid. On reading temperature when the meniscus vanishes in the former case, it will appear higher than in the latter. The difference is accounted for by the fact that during evaporation, the temperature of the copper block is continually increasing. This would point to the conclusion that a considerable amount of heat must be absorbed, even under such circumstances, in order to convert liquid into gas, and thus that the latent heat of vaporisation is still considerable, even at temperatures so near the critical point.

The critical change for benzene occurs at a temperature of 291°·7, and at a pressure of 60·3 to 60·5 atmospheres, and the isotherm at this point is represented in Curve VII of Table A. Between the volumes of 90 and 60 it is evident that gas is being compressed, for pressure rises regularly. But from 60 to 38 the rise of pressure required to produce diminution of volume is smaller proportionately to the effect produced, and after the volume 38 has been reached the pressure rises much more rapidly. And, on referring to Table D and to the accompanying diagram, it is also to be remarked that, when the volume is 60, or greater, the curve of equivolume represents the total evaporation of the substance. With a volume of 50, that particular proportion of gas to liquid appears to be reached at which evaporation almost exactly balances expansion, and neither total evaporation nor total expansion takes place, but the ratio of gas to liquid appears to remain unaltered. With volumes of 40 and 35 divisions of the tube total expansion takes place, and the tube, above a certain temperature, *must* become filled with liquid. Above the temperatures represented in Table D, it becomes impossible to distinguish liquid from gas, for the meniscus has disappeared. But I can see no reason for assuming a particular state of matter under such circumstances. In the preliminary note already referred to, I described an experiment, in which liquid and gas were kept separate for some time by means of a capillary tube, and in which even after the meniscus of the liquid had disappeared, a solid, adhering to the wall of the tubes containing presumably only gas, refused to dissolve. This experiment has been frequently repeated, with identical results; it is, perhaps, most striking when the fluorescent colouring matter, eosine, is used as the solid. Eosine fluoresces only

when in solution; when dry, it is a red powder; and when alcohol containing eosine in solution is placed in one compartment of such a tube, the other having its sides coated with a thin film of dry eosine, no fluorescence takes place till time has been given for diffusion. In fact, the rate of diffusion may be approximately measured by the increase in intensity of fluorescence in that half of the tube originally containing the vapour of the solvent.

Three facts appear, therefore, to be demonstrated:—First, that at the temperature at which the meniscus of a liquid disappears, and at temperatures above that point, but not far removed from it, an increase of pressure is required to cause diminution of volume, comparable with that necessary to compress a liquid at a temperature somewhat below that at which its meniscus disappears; second, that when a mixture of liquid and gas is maintained at a certain volume, the expansion of the liquid on raising the temperature, *so long as it is possible to distinguish liquid from gas*, points to the ultimate occupying of the whole space by liquid at temperatures above which the meniscus becomes invisible; and third, that under such circumstances the liquid retains its solvent powers, while the gas is incapable of dissolving a solid. All these facts point to the conclusion, that at or under such a volume the matter is really in the liquid state, whereas at a greater volume, the matter must be viewed as consisting at least partially of gas.

No direct experiments have been made with a view to ascertaining whether heat is evolved when a gas is converted into liquid by pressure at such high temperatures. I hope to be able to execute some experiments which promise some satisfactory answer to the question.

It now remains to consider the condition of raising a mixture of two liquids to such a temperature that the meniscus disappears. Isotherms for a mixture of benzene and ether are given on Table C, and graphically represented on the diagram.

The first isotherm at the temperature 215°·6 is at least 20 degrees above the temperature at which the meniscus of pure ether disappears, and yet the tension of ether vapour does not markedly appear. If that curve be constrasted with Curve No. VI for ether alone at 207°·1, some 8 degrees lower, it is noticeable that diminution of volume in the latter case is accompanied by a much greater rise of pressure than in the former. The presence of benzene, therefore must exert some marked influence on the pressure exercised by ether vapour, and cause the mixture to behave to some extent as a single substance. But at higher temperatures the influence of the ether becomes more marked, and at the temperature 240°·7 the critical point is nearly reached. The tube then appeared full of mist, till the volume 40 was reached, when the mist disappeared, and the tube appeared full of liquid. The pressure at which ether becomes critical is situated about

40 atmospheres; that at which benzene reaches the critical state 60·5, and that of the mixture 48. The temperatures are: ether, 195°·5; mixture, 240°·7; benzene, 291°·7. Both temperature and pressure thus appear to take a position not far removed from the mean of the two.

The definition of the words liquid and gas appears to require more accuracy than has hitherto been bestowed. As no known æriform body absolutely obeys the law of contraction inversely as the pressure, and equal expansion on equal rise of temperature, there is apparently no instance of a perfect gas, although this state is closely approached by such gases as hydrogen, oxygen, and carbonic oxide, especially at high temperatures and not too great pressures. And the definition of a liquid appears to be a fluid exhibiting surface tension. Now, above the critical point, this surface tension disappears as has been repeatedly shown. But I venture to think that the possession of surface tension is not a criterion of the existence of a liquid. And a most striking argument in support of this theory has lately been furnished by M. Cailletet ("Compt. Rend.," xc, 210). He found that carbonic anhydride at a temperature of 5°·5, when the lower portion of his experimental tube was filled with liquid, the upper portion being filled with a mixture of gaseous carbonic anhydride with air, mixed with the air when a pressure of 130 atmospheres was applied. The question is a simple one; does the gas become liquid, or the liquid become gas? Or do they both enter a state to be called neither liquid nor gas?

I venture to bring forward a theory, with great diffidence, which appears to be supported by numerous observations, viz., that there exists a close analogy between the condition of liquid as compared with its gas, and of a compound as compared with the elements of which it is constituted, and that in the evaporation of a liquid we have to do with a true instance of dissociation, that is, a decomposition of complex molecules into simpler ones. Many compounds, when heated, dissociate into their elements, or into simpler compounds. The extent of dissociation is a direct function of the temperature, and an inverse function of the pressure. Thus, ammonium chloride, when heated, dissociates into ammonia and hydrogen chloride; hydrogen iodide into iodine and hydrogen. It is evidently possible so to regulate temperature and pressure as to obtain a mixture of hydrogen iodide with hydrogen and iodine in any desired proportion. If the analogy holds, it is possible to obtain a mixture of liquid molecules with gas molecules in any desired proportion; but as surface tension appears to be permanent until liquid and gas reach the same density, mixture does not occur before that point. Still, mixture may be held to exist to some extent, for the vapour is not a perfect gas, and this is probably owing to its containing some liquid molecules among its gaseous ones.

The question is also closely connected with that of heat of vaporisation and heat of combination. It is possible to exhibit this point more clearly by help of an example. The heat of vaporisation of water, under a pressure of 760 millims. is 513 calories for 1 cub. centim. at 100°. The expansion which the liquid undergoes in becoming gas is represented by the number 1623. From the known equivalent of heat in work, it is easy to calculate the total work necessary to evaporate water; and also the work required to expand the substance 1623 times against atmospheric pressure. The work done as heat, in the case of water is 221·1 kilogram-metres; and as expansion, 16·6 kilogram-metres: hence 221·1 — 16·6 is work done in overcoming molecular resistance. But this work is infinitely more than is necessary to overcome surface tension, and the most probable conjecture is, I venture to think, that the work is employed in dissociating the complex molecules of water into simpler molecules of water-gas.

Now, in the foregoing paper, experiments have been described which show that when a liquid is heated in a certain confined space, the results of observation, possible while the liquid is still distinguishable from its gas, lead to the conclusion that at a temperature at which the meniscus of the liquid has disappeared, total expansion *of the liquid* will take place, and that a certain larger volume, total evaporation will ensue.

It may be objected that Regnault's measurements of the heat of vaporisation of liquids at high pressures appear to show that it is a quantity diminishing with the temperature. But it has never been shown to be the contrary in the case of heat evolved during chemical combination. Is it not likely that there will be a less evolution of heat during the combination of hydrogen and iodine at a high than at a low temperature and pressure? And to return to M. Cailletet's experiment, is it likely that compression, which, as a rule, has the result of turning gas to liquid, should in this case change liquid to gas?

To sum up: the views expressed in this paper are:—(1) That a gas may be defined as a body whose molecules are composed of a small number of atoms; (2) a liquid may be regarded as formed of aggregates of gaseous molecules, forming a more complex molecule; and (3) that above the critical point, the matter may consist wholly of gas, if a sufficient volume be allowed; wholly of liquid if that volume be diminished sufficiently; or of a mixture of both at intermediate volumes. That mixture is, physically speaking, homogeneous, in the same sense as a mixture of oxygen and hydrogen gases may be termed homogeneous; but chemically heterogeneous, inasmuch as it consists of molecules of two different natures. When prevented from mixing by interposing a capillary tube between the two, the liquid and gas retain their several properties.

III. "Further Researches into the Colouring-matters of Human Urine, with an Account of their Artificial Production from Bilirubin, and from Hæmatin." By CHARLES A. MACMUNN, B.A., M.D. Communicated by Dr. MICHAEL FOSTER, Prælector of Physiology in Trinity College, Cambridge. Received November 10, 1880.

In a former paper which I had the honour of laying before the Royal Society, I endeavoured to describe the spectroscopic and some of the chemical characters of febrile urobilin.

In the present paper I have given the results of further spectroscopical research, which had for its object:—(1.) To determine the differences which might exist between those urinary pigments which are recognisable by means of the spectroscope, in health and disease. (2.) The isolation of normal urinary pigment, giving the band at F. (3.) To attempt to trace back to their source all these pigments. (4.) To examine bile more carefully for the presence of urobilin. (5.) To find an explanation of the absorption-bands noticed in the bile of certain animals.

In this paper I shall give principally the spectroscopic appearances of these pigments, reserving for a future communication a full description of their chemical characters. Speculation will be avoided as much as possible, and a plain statement of the facts which presented themselves will be adhered to, which show that there is irresistible evidence of the relationship between the colouring-matters of blood, bile, and urine.

It is probable that a knowledge of how the urinary pigments can be prepared artificially will be of great use in enabling us to understand how they are produced in the body. Thus the knowledge of the fact that the spectrum of urobilin varies according to the amount and the kind of oxidation, or reduction, or both, to which it has been subjected in the body, which I shall endeavour to show is the case, is of great importance, especially as we can produce in the laboratory pigments (from bile- and blood-colouring matter), by a greater or a less amount of oxidation or reduction, or of both combined, which can be made to resemble exactly pigments obtained from urine in health and disease.

The Spectrum of Normal Urine: Its Band due to the presence of a Pigment indistinguishable from Choletelin.—In examining urine obtained from individuals in a healthy condition, I always can see a band at F, and when a layer sufficiently deep to show this band is treated with caustic soda, caustic potash, or ammonia, the band can be no longer seen. On the subsequent addition of an acid, it is again brought into view. But if the urine be obtained from a febrile case, or indeed,

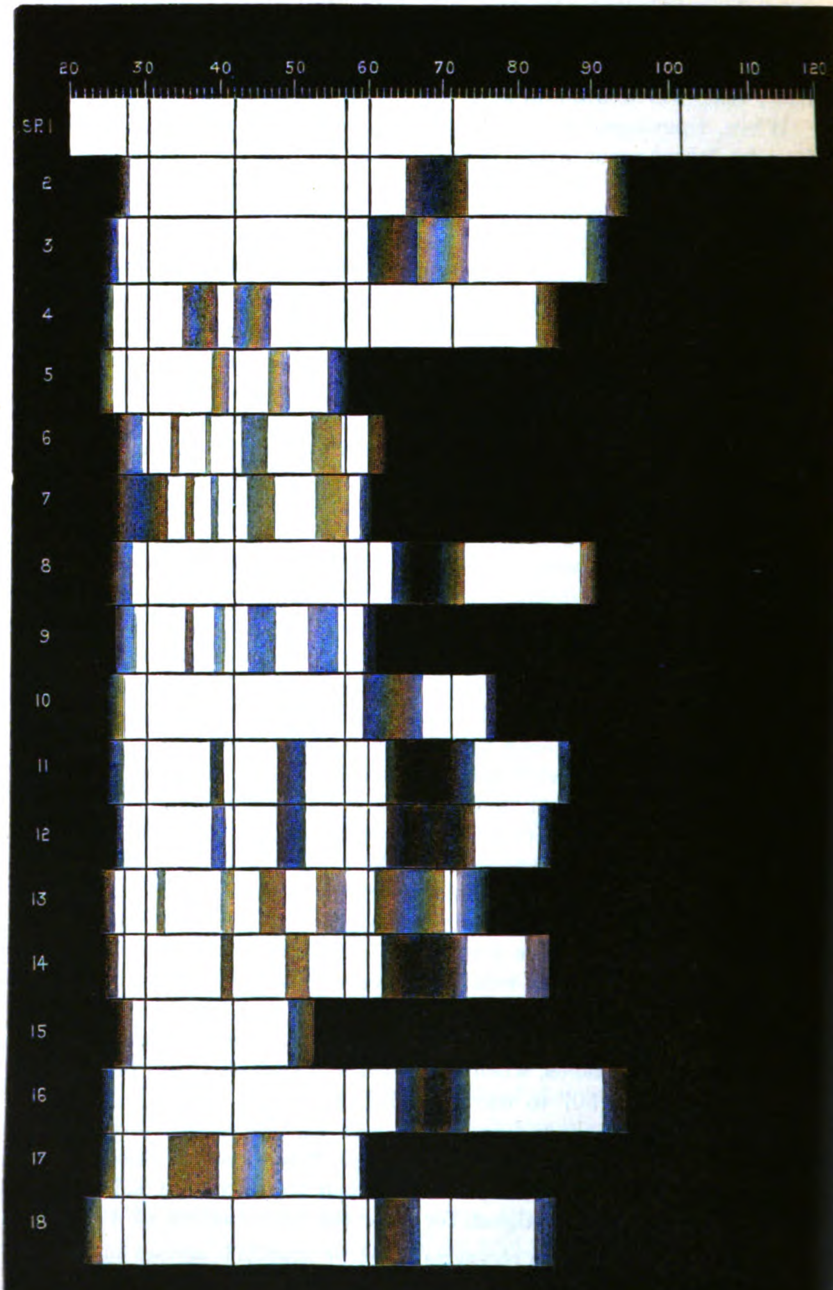
from a case where there may be but a slight departure from the normal condition, the band at F is replaced by a band nearer the red, when these reagents, caustic soda or caustic potash, are added; but ammonia causes its disappearance. The reason of this is, that in the latter case, *febrile* urobilin is present.

When, therefore, the pigment which gives the band at F is isolated from healthy human urine, it should present the same spectroscopic characters as the urine containing it. Such is the case: for when normal chrome-yellow coloured urine is precipitated with neutral and basic acetate of lead, the precipitate extracted with alcohol acidulated with sulphuric acid, the acidulated alcohol containing the pigment separated by filtration from the lead precipitate, the fluid diluted with water and shaken with chloroform in a separating funnel, the chloroform separated, and then distilled off, a residue is left, which is a *brown-yellow*, amorphous, nitrógenous pigment, soluble in alcohol, ether, chloroform, and benzol, also in acids, and which gives in its various solutions the same band that was seen in the urine, and altered in the same manner by reagents, as it was altered in that fluid. While *febrile* urobilin gives a sharp black band at F of intensity α, the band of *normal* urobilin is less marked at its edges, and is less shaded than the former. Its alcoholic solution shows the band well, and when this is treated with caustic soda, caustic potash, or ammonia, it disappears. Sometimes its disappearance may not be complete, and in that case, the pigment which I named urolutein in my former paper may be present. The pigment may sometimes appear more brown than brown-yellow in colour, and in that case it shows a tendency to imitate febrile urobilin in its behaviour with the caustic alkalies, for on their addition a feeble band may be noticed nearer the red than the original band. More especially is this likely to occur if the acidulated alcohol contains too much acid, or if the fluid be left too long in the separating funnel.* As a general rule, the more the colour of the pigment approaches to brown, or brownish-red in colour, the more does it resemble febrile urobilin in its characters. The colour depends upon the amount of oxidation to which it has been subjected in the body, as well as on its artificial preparation, as I shall endeavour to show afterwards.

The band of normal urobilin is shown in Chart I, sp. 2. As a general rule, it extends, when examined in a suitable depth of alcohol, from wave-length 507 to wave-length 482. While alcoholic solutions of febrile urobilin are red in colour, and become yellow with caustic alkalies, the alcoholic solution of this pigment becomes redder with caustic soda. This is well marked when the alcoholic solution is treated with sodium amalgam, for after the introduction of this sub-

* Because in that case the *chromogen* of *febrile* urobilin is oxidised into febrile urobilin. (*Vide infra.*)

Chart I.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stance, the colour of the fluid becomes orange-red, and general absorption of the violet end of the spectrum takes place. This reaction at once shows a likeness between this pigment and choletelin, but even a more striking likeness is exhibited by the action of chloride of zinc, for when the pigment in its alcoholic solution is treated with chloride of zinc, the colour of the fluid at once gets redder, and it then shows a narrow and sharp band nearer the red end of the spectrum. The edge of this band nearer the red is the more abruptly shaded, that next the violet shows a gradually decreasing shading. Thus, taking an actual experiment:—

Band before zinc chloride, wave-length 504 to wave-length 484.
Band after zinc chloride (sp. 3).

Dark part of band, wave-length 516 to wave-length 501.
Feeble shadow up to wave-length 484.

If, now, caustic soda be added to the fluid treated by zinc chloride, the precipitate being dissolved in an excess of that reagent, the fluid becomes *yellow*, and the same band as that got by treating febrile urobilin is then seen, of intensity δ. Now Heynsius and Campbell* found that choletelin acted in the same manner, for it could not be made to give this band δ, until it had been first treated with zinc chloride.

The following differences collected together were found to exist between normal and febrile urobilin:—

(1.) The acidulated alcoholic extract of the lead precipitate is lighter in colour than that of febrile urobilin.

(2.) The chloroformic solution is of a yellow colour, and when poured on a Berlin dish it is seen to be slightly reddish where its edge touches the dish. The same solution of the febrile urobilin is red.

(3.) The absorption-band in the urine, in alcoholic solutions and in chloroformic solutions, has less well-defined edges and is less shaded than that of febrile urobilin.

(4.) The band at F is made to disappear by means of caustic alkalies, while it is replaced by another in the case of febrile urobilin.

(5.) The pigment is yellow-brown, febrile urobilin being reddish-brown.

When sodium amalgam is put into an alcoholic solution, as previously mentioned, the colour becomes orange, but by continuing the action longer, then acidulating with hydrochloric acid and shaking with chloroform, and evaporating off the chloroform, I obtained a brownish pigment, which, when dissolved in alcohol and treated with caustic soda, gave a band on each side of D (Chart I, sp. 4). Now, it is a remarkable fact, that I subsequently observed the same bands on

* "Centralblatt f. d. Med. Wiss.," 1872, p. 696.

treating febrile urobilin, obtained from a case of pleurisy, with caustic soda (Chart I, sp. 17), and I think this points to the conclusion that normal urobilin has a tendency to pass, when reduced by sodium amalgam, into the condition of febrile urobilin.* Moreover, these bands indicate the source of the pigment in the economy, as similar bands are seen in the spectrum of a pigment obtained from gall-stones (Chart II, sp. 2), in that of the alcoholic extract of bile-colouring matter, and also in that observed when hæmatoin was reduced by means of sodium amalgam in the neutral state at the ordinary temperature, and the fluid examined at an early stage of the reaction.

A faint band covering D may sometimes be seen in solutions of normal urobilin, but I have not yet determined upon what conditions its presence may depend. (See, however, Chart II, sp. 15.) It can only be seen in deep layers of alcoholic solution. The amount of normal urobilin in urine is small, but what I have been able to obtain after about thirty experiments will suffice to establish its identity.

This pigment has, up to the present time, been confounded with febrile urobilin, but it will be seen that it is quite a different body. Before I had succeeded in isolating it, I had concluded that it was identical with febrile urobilin, and since this normal urobilin is identical with choletelin, and since the latter pigment is produced by oxidation from bilirubin, I had concluded that febrile urobilin was produced by oxidation. It would appear that *febrile* urobilin, although it may represent an *intermediate stage* of the oxidation of bilirubin, is capable of being produced by reduction of choletelin, and therefore of *normal* urobilin, and also of a similar body produced by the oxidation of hæmatoin by peroxide of hydrogen. We may conclude that *febrile* urobilin is the same body as that obtained by Maly, and which he called hydrobilirubin, but that the present pigment is an entirely different body, and is produced by oxidation. But there is another body present in urine which is capable of passing into the condition of *febrile* urobilin when strong oxidising agents are made to act on the urine, in fact, it may be accepted as an established truth, that *the chromogen of febrile urobilin exists in normal urine*. Disque† believes that it is this body which furnishes urobilin when urine is treated with acids, and that it is oxidised in the presence of chloroform into that body. Such may be true in some cases, but not in all, as the following experiment will show. When a stream of chlorine is passed through perfectly normal urine, or when this fluid is treated with permanganate of potassium, bromine in aqueous solution, or ozone, the colour soon changes to yellowish-red, and a black band is seen at F. When caustic soda is added after such treatment, this

* Again, they may be seen when *normal* urobilin has more of a brownish tinge, by treatment with caustic soda *alone*.

† "Chem. Centr.," 1878, s. 711.

band is replaced by another nearer the red end of the spectrum, as in the case of febrile urobilin. Now, from another part of the same urine, *which has not been thus treated*, we can, by precipitation with lead acetate, and subsequent treatment with acidulated alcohol and chloroform, obtain *normal* urobilin. It therefore appears that it is not by the oxidation of the chromogen of febrile urobilin* that normal urobilin is obtained, but that this body (*i.e.*, normal urobilin) is present in the urine *as such*, or part may be present *as its own chromogen.* That such is the case will appear to be likely, when I come to describe the artificial production of normal urobilin from hæmatin.

Urohæmatin. A Pigment excreted in the Urine of a case of Subacute Rheumatism.—The patient in whose urine this pigment occurred, was suffering from subacute rheumatism, and was taking 15 grs. of the salicylate of soda three times a-day. This pigment is of great interest as it can be prepared with ease artificially from hæmatin; and as it appears to be incapable of production from bilirubin, I have named it urohæmatin. The urine was a dark reddish-yellow colour, but did not contain blood or bile pigments as proved by appropriate tests. It gave a black band 63—74, or wave-length 507 to 480; with caustic soda, this band was replaced by another of intensity β or γ, from wave-length 513 to 491. No other bands were noticed in the urine itself.

1,000 cub. centims. of the urine were taken and precipitated with neutral and basic acetate of lead, and afterwards treated in the same manner as that already described,† for the isolation of normal urobilin. The chloroformic solution gave the remarkable spectrum seen in Chart I, sp. 6, and was the colour of dark golden sherry. In a thinner layer another band, α, made its appearance, reading wave-length 507 to 484. (Cf. sp. 8.) When the chloroform was distilled off, the residue was seen to be a dark-brown colour, and was soluble in alcohol, giving a red solution, and sp. 7, Chart I, and sp. 8.

Ammonia did not cause the disappearance of the band at F when added to the alcoholic solution, but acted like caustic soda, namely, by causing another band, nearer the red than was the original one at F, to appear.

Caustic soda made the fluid orange in colour, and shifted some of the bands very slightly, as shown in Chart I, sp. 9, but the replacement of that at F was well marked, sp. 10.

* It is this chromogen which becomes oxidised when urine begins to decompose, so that stale healthy urine may contain febrile urobilin as such, and give its spectrum.

† The acidulated alcohol extract gives almost the same spectrum as that got by treating artificially prepared normal urobilin, reduced by means of sodium amalgam, with sulphuric acid. (See sp. 5, Chart I, and cf. sp. 11, Chart I, and sp. 12, Chart I, and Chart III, sp. 17.)

The pigment was slightly soluble in ether and in benzol, but insoluble in bisulphide of carbon.

Hydrochloric acid and water dissolved the pigment completely, and a different spectrum was then seen (Chart I, sp. 11).

Strong sulphuric acid dissolved the pigment, forming a red solution, giving sp. 12, Chart I.

Permanganate of potassium did not seem to affect the spectrum, but peroxide of hydrogen seemed to remove the feeble bands, leaving a shadow from wave-length 584 to 567 and a band, α, wave-length 507 to 482.

Sulphurous acid made the alcoholic solution lighter in colour, and gave in deep layer almost the same spectrum as with sulphuric and hydrochloric acids; and in a thinner layer, the band at F was the same as before its addition. *Hyposulphite of sodium* did not affect the spectrum.

When the alcoholic solution was treated with *sodium amalgam*,* its reddish-brown colour changed to pale yellow after fourteen hours' action, and it then gave sp. 13, Chart I. When this yellow fluid was cautiously neutralised, and then slightly acidified with hydrochloric acid, it became redder in colour and then gave sp. 14, Chart I, in a suitable depth.

When the fluid treated by sodium amalgam, and subsequently hydrochloric acid, was treated by permanganate of potassium, the band at F was made fainter, and did not appear to be replaced by another when caustic soda was added.

This pigment was darker brown than febrile urobilin, which has a reddish-brown colour, and it was evidently nearer to acid hæmatin than the latter pigment. Its affinity to the latter pigment was shown by the way in which its band at F was affected by caustic soda, but by the way in which that band was affected by ammonia it was seen to be different.

By the action of zinc and sulphuric acid on acid hæmatin, I have succeeded in obtaining the same pigment (as will be described further on), and the solutions of the artificially prepared pigment gave the same spectra as those of this one, band for band, and the spectra of its various solutions were altered in the same manner as those of the present one by reagents. It would therefore appear that the various bands seen in solutions of urohæmatin are all due to one pigment, and not to the presence of impurities.

Urobilin, from the Urine of a case of Pleurisy, probably due to Tuberculosis.—There was but slight effusion into the pleural cavities; the temperature of the patient was 101° F. The urine was reddish-yellow in colour, contained neither bile or blood, and gave a black band at F,

* Cf. the action of sodium amalgam on hæmatoin, *infra*.

which was replaced by another one less shaded and nearer the red when caustic soda was added.

360 cub. centims. of the urine were precipitated with neutral and basic acetate of lead and treated as before. The acidulated alcoholic extract of the lead precipitate gave in deep layers no bands in red or orange. This alcoholic extract was red in colour. In shallow layers a black band was seen at F (Chart I, sp. 15 and sp. 16).

The chloroformic solution was reddish-yellow and gave a black band at F,* and a feeble shadow extending for about the breadth of the band itself on its redward side. In deep layers no other bands could be seen. The black band α read from wave-length 501 to 482. The pigment left after the evaporation of the chloroform was reddish-brown in colour, and behaved like febrile urobilin as to solubility.

Alcohol dissolved it, giving a reddish-yellow solution, which with caustic soda became perhaps slightly *redder*, and it then gave in deep layers sp. 17, Chart I, and in shallow sp. 18. These two bands at D were seen when normal urobilin was treated with sodium amalgam, as already referred to. (*Vide supra.*)

With chloride of zinc another band appeared nearer the red in the position of that produced by caustic soda. This pigment did not give the same characters as normal urobilin, nor yet did it give exactly those of febrile urobilin. From the appearance of the bands near D with caustic soda, and taking into consideration the fact that these bands were noticed when normal urobilin was reduced with sodium amalgam, I believe the conclusion follows that this pigment was less oxidised than normal urobilin and less reduced than febrile urobilin.

I have selected this pigment to show that the statement made in my former paper was correct, and that urobilin† appears capable of existing in different states of oxidation. I have come to the conclusion that the greater the number of feeble absorption-bands noticed, the less the oxidation to which the pigment has been subjected in the body. As a type of a pigment which has been produced by reduction only, I may refer to urohæmatin.

Preliminary Experiments on the Oxidation of Bile Pigments.—In attempting to trace back normal and febrile urobilin to their origin, one naturally begins with bilirubin; consequently my first experiments were made on solutions of bilirubin, obtained by treating human bile with alcohol to precipitate the mucus, and then, after filtration, shaking with chloroform. I was not aware that I was dealing with solutions which might also have contained urobilin; in fact, I had come to the opposite conclusion, since such solutions failed to give a band at F. But when one considers that, even in spite of the absence

* It was noted that the shading on the redward side of the band at F was only seen in chloroformic solutions.

† This remark applies to pathological pigments more especially.

of that band, urobilin might be present—for its band is invisible in slightly alkaline or neutral solution, and even after shaking with chloroform it may still be invisible until after an acid has been added—the conclusion follows that what has been supposed to be due to the transformation of bilirubin into urobilin may, after all, be nothing more than the gradual appearance under oxidation of the band of a pigment already existing in the solution. The behaviour of even impure solutions such as these is, however, very instructive. When a chloroformic solution is put into a stoppered bottle, so as to fill it about one-third, and the bottle is shaken from time to time, the fluid gradually gets light in colour. The general absorption of the violet end of the spectrum characteristic of bilirubin gradually gives way to an interesting special absorption, which is characterised by the appearance of a band on each side of D, which is soon followed by the appearance of one at F. Then the former bands fade gradually away, leaving the band at F. This change was found to have been completed at the end of three weeks, and the colour of the fluid was then a pale brownish-yellow with transmitted daylight. The same series of changes in the spectrum accompanies the play of colour got by the action of nitric acid on bilirubin, and at the penultimate stage of the reaction, at the brown-yellow stage, we can isolate the pigment giving the band at F. But if the reaction be allowed to go on further, the fluid becomes almost colourless, and no longer can the band at F be seen. Isolated at the penultimate stage, the pigment is found to be choletelin, but it is evident that if the bilirubin thus treated contained urobilin, accurate inferences cannot be drawn from the characters of the pigment isolated, as it might be not only an oxidised pigment derived from bilirubin, but also a decomposition product of urobilin.

Preparation of Pure Bilirubin.—Accordingly, it became necessary to procure pure bilirubin, which was done according to the directions given in the excellent "Handbook for the Physiological Laboratory" of Professor Burdon-Sanderson. Brown human gall-stones were powdered, extracted with ether, the residue boiled with water and treated with diluted hydrochloric acid. After washing and drying, the mass was boiled with chloroform; the chloroform distilled off over the water-bath; the residue treated with absolute alcohol. It was then treated with ether and alcohol repeatedly, and again dissolved in chloroform, from which it was precipitated by absolute alcohol. The pigment thus obtained was an amorphous orange-coloured powder.

It is not possible in the limits of this paper to describe all the reactions and the spectra of the solutions obtained by this treatment of the gall-stones, so that I shall only refer briefly to them as they bear upon the subjects discussed here. The first *ether extraction* of the gall-stones gave two bands, which are evidently those of lutein—γ from wave-length 482 to 469, and δ from wave-length 459 to 442.

The *hot water* extraction of a brownish colour gave a band, δ, from wave-length 507 to 486. The *acidulated water* also gave a faint band in the same part of the spectrum. The *alcoholic extract* gave a band on each side of D, which latter were evidently similar bands to those noticed in solutions of the urobilin of pleurisy treated by caustic soda, or in solutions of normal urobilin treated by sodium amalgam, and in the intermediate stage of Gmelin's reaction, and in the alcoholic extract of human and sheep-bile pigments, and which can be prepared artificially by the action of sodium amalgam, *in the cold*, on solutions of hæmatoin (when the pigment has been separated in the neutral state). This alcoholic solution was of a red colour with transmitted light in deep layers, while it was yellow in thin layers. One band, γ, extended from wave-length 620 to 592, the other, δ, from wave-length 585 to 569, sp. 2, Chart II. With ammonia this fluid gave the spectrum seen in Chart II, sp. 3. The occurrence of these latter bands shows that the gall-stones contained urobilin,* as similar bands are seen by similar treatment of urobilin when it is obtained from bilirubin by sodium amalgam, as will be referred to again.

Action of Chlorine on Pure Bilirubin.—The colours and changes of spectrum, similar to those which accompany Gmelin's reaction, can be studied with great ease by passing chlorine, well diluted with oxygen (such as may be obtained in traces when black oxide of manganese is heated with chlorate of potassium), into a chloroformic solution of pure bilirubin. In such a solution this reagent brought about the following changes in the colour and spectrum. The colour of the original solution being orange, it soon changed to:—

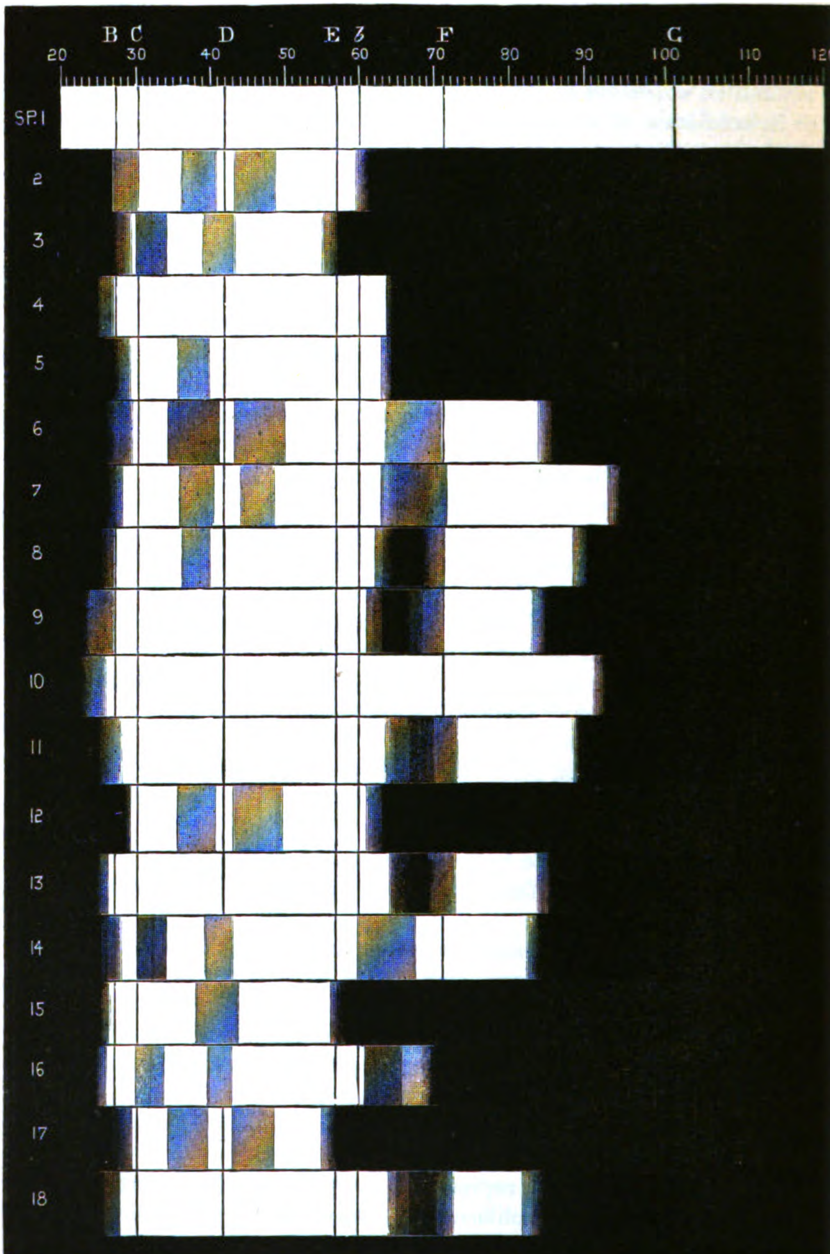
1. Greenish-yellow.
2. Sap-green (band before D, 625 to 598 wave-length).
3. Dark sap-green.
4. Green.
5. Bluish-green (band before D and traces of another after D).
6. Dark blue-green (band before D, and band from wave-length 588—546).
7. Indigo-blue (two bands, as before, and slight shading at F).
8. Indigo (band β, wave-length 620—598; ε, 588—555; α, 504—482).
9. Purplish-blue (band at F black; ε, fainter).
10. Lilac (band at F black, ε almost gone, and β faint).
11. Port-wine red (β fainter; α as before).
12. Reddish-yellow (α getting fainter, others gone).
13. Light yellow (no band to be seen).

These appearances are represented in sp. 4 to sp. 10, Chart II.

When pure and dry chlorine, prepared in the usual manner and

* *i.e.*, urobilin of *biliary* origin.

Chart II.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purified by being passed into a solution of sulphate of copper, then strong sulphuric acid and, lastly, U-tubes containing chloride of calcium, is made to pass through a solution of bilirubin in chloroform dried by chloride of calcium for some days previously, the colour of the fluid changes much more rapidly, but the changes in colour are accompanied by the same changes in the spectrum already noticed.

On the Reduction of the Pigment present in the last stage to the condition of Febrile Urobilin by means of Sodium Amalgam.—It is evident that the changes which take place are similar to those which are seen in Gmelin's reaction, that at the penultimate stage choletelin is formed, and that even at the last stage this is also further oxidised, as proved by the complete disappearance of the band at F. If choletelin be a fully oxidised bile pigment and febrile urobilin a less oxidised bile pigment we should be able to reduce choletelin back to febrile urobilin by the action of reducing agents. Accordingly, I proceeded to isolate the pigment of the yellow stage, having first filtered the chloroformic solution. It was then evaporated on the water-bath. The residue was a light yellowish-brown pigment, perfectly soluble in alcohol with a yellow colour. I could not see a band at F, for the pigment had been oxidised beyond the stage at which it gives this band. When a piece of sodium amalgam was introduced into the alcoholic solution the latter immediately became of a reddish colour. After it had acted for a short time the fluid was found to have the power of absorbing the violet end of the spectrum. When hydrochloric acid was added to the solution *before* the action of sodium amalgam no change took place, but when it was added to the red fluid *after* the action of the amalgam, it gave a black band, a, wave-length 506 to 481, sp. 11, Chart II. And when caustic soda was added until an alkaline reaction was developed a band, δ, appeared from wave-length 513 to wave-length 488.

After the action of the amalgam had gone on for twenty-four hours the colour of the fluid was light yellow, and hydrochloric acid then produced a reddish fluid giving a band of intensity, a, from wave-length 502 to wave-length 478.

But this pigment had gone just beyond the stage of choletelin, so that it became necessary to prepare that pigment. I thought that by preparing it by another method I should have additional evidence of the truth of the idea that it can be made to yield febrile urobilin by reduction, if the result should turn out favourably.

Preparation of Choletelin from pure Bilirubin and its Conversion into Febrile Urobilin.—Some pure bilirubin, prepared as before, which gave only general absorption in the deepest and thinnest layers when dissolved in chloroform, was treated with a little caustic soda in a chloroformic solution and exposed to the air in an evaporating dish. After the lapse of twenty-four hours the residue was found to be sap-

green in colour, and it was then dissolved in alcohol. The solution was then filtered so as to catch any unchanged bilirubin. The filtrate was then seen to be a brilliant sap-green colour and gave only general absorption of the spectrum. It was now treated with strong nitric acid, and examined with the spectroscope. When the bands on each side of D had completely disappeared, leaving one at F of intensity β or γ from wave-length 507 to 482, the solution was shaken with chloroform in a separating funnel, and the reddish-yellow chloroform layer was separated off, and filtered. After evaporation of the chloroform, a brownish-yellow, or yellowish-brown amorphous pigment was left, soluble in the same solvents as normal urobilin. This pigment, when dissolved in alcohol, gave a yellow solution, and when looked at in a white dish, it had a slightly reddish tinge at the edge, where it touched the white surface of the dish, this being better marked in a chloroformic solution. The alcoholic solution gave a band of intensity β, from wave-length 510 to 482, having ill-defined edges. When the fluid was treated with caustic soda, it became of an orange colour, and then general absorption of the violet end of the spectrum was noticed. In a moderately deep layer, the dark shading commenced at wave-length 510. No band could be seen in a thinner layer. When caustic soda was added after the addition of zinc chloride, the reddish colour produced by the zinc chloride became yellow, and I then perceived a feeble band from wave-length 516 to 488, but it was difficult to take the reading of this band. Sodium amalgam produced exactly the same effect that it produced with the pigment got by the action of chlorine, and the description given before will apply word for word to the present pigment.

Action of Ozone on Pure Bilirubin.—Fearing that nitric acid might not have produced the pigment by oxidation, I planned an experiment by which ozone was made to act on bilirubin dissolved in chloroform. A Siemens induction tube* was made by taking two test-tubes, one larger than the other; the inner surface of the small tube was coated with tin-foil, and the outer surface of the larger one. They were kept from touching each other by four small points of sealing-wax when one was placed within the other. All the space between the tubes was closed, except a hole at either end of the larger tube, into which a small glass tube was fastened. So that I had two concentric tubes, coated, the inner one on the inside and the outer one on the outside, with tin-foil, and containing a space between them through which oxygen could be passed. The coatings were respectively connected with the terminals of a Ruhmkorff's induction coil, worked by a quart bichromate cell. When oxygen was then passed into one end of the space between the tubes, it came out ozonised through the small

* "Bloxam's Chemistry," 4th ed. (1880), p. 53.

delivery tube at the other end. The latter was allowed to dip almost to the bottom of a test-tube containing a chloroformic solution of pure bilirubin. The oxygen was purified by being passed through strong sulphuric acid before entering the induction tube. After the ozone had been passed into the solution for fifteen minutes, it got slightly redder in colour, and it then gave a band covering D; no other band in shallow layer. The general absorption of the violet disappeared gradually, and a band δ became detached at F, the colour of the solution becoming lighter at the same time. After longer action, the colour became still lighter, and the band still remained. As no other change took place, the action of the ozone was discontinued. The band at F δ read from wave-length 513 to 482, and when hydrochloric acid was added, it got darker, and gave the same reading, the colour of the fluid becoming red. But when caustic soda was added, the band read 507 to 480, and did not disappear. The pigment formed was therefore *not* choletelin, for its band should have disappeared with caustic soda, and if it had been febrile urobilin, it should have been displaced towards the red, instead of which it came nearer the violet. But although the pigment produced by the action of ozone on bilirubin was neither (apparently) choletelin nor febrile urobilin, yet its action was somewhat similar to other oxidising agents, in causing disappearance of the general absorption and the formation of a pigment giving a band at F.

Action of other Oxidising Agents on Bilirubin, &c.—The action of permanganate of potassium and peroxide of hydrogen on bilirubin is not easily studied, from the difficulty experienced in getting them to act on bilirubin; for in chloroformic solution they will not do so, and when made to act on solid bilirubin, their action is confined to the surface, but on the whole, their tendency is to convert this pigment into choletelin. Peroxide of hydrogen, when added to bilirubin undergoing oxidation, seems to advance the oxidation a stage, and then stops short. Although it acts with difficulty on bilirubin, there are other biliary pigments, such as those got in the alcoholic extract of human bile pigments, with which peroxide of hydrogen gives a play of colours, accompanied by the same alteration of spectrum which accompanies Gmelin's reaction; but this will be referred to again.

*Action of Sodium Amalgam on Pure Bilirubin.**—Bilirubin was suspended in water, and a piece of sodium amalgam introduced. After a few minutes a little of the fluid was taken and treated with hydrochloric acid, which caused the formation of brownish flakes; these were soluble in alcohol, forming a yellow fluid giving *only general absorption.* At the end of an hour, the fluid was brownish in colour, but lighter than it was at the end of half-an-hour. After nine and a

* "Ann. Ch. Pharm.," clxi, 368; clxiii, 77, contain an account of Maly's experiments on this subject.

half hours it was yellow in colour, and minute particles of a brownish substance were seen suspended in it. This yellow fluid gave a band, δ, from wave-length 513 to 488. It was then treated with hydrochloric acid, until acid in reaction, when it became reddish-brown. It was then filtered as brownish particles became separated by the action of the hydrochloric acid. The filtrate was a beautiful red colour, and gave a black band, α, 507 to 480; another reading in a thinner layer gave wave-length 501 to 482. When caustic soda was added to alkalinity another band, intensity δ, appeared from wave-length 513 to 486, the solution at the same time getting yellow in colour.

The insoluble portions in the filter were a dirty green-brown colour, and gave, when dissolved in alcohol, an olive-coloured solution. In deep layers of this alcoholic solution there were seen two bands near D, the darker before D, the lighter on its violet side, and in shallow layers a dark band, α, was seen at F. (See Chart II, sp. 12 and 13.) Treated with caustic soda sp. 14 appeared. I may here mention that these same bands appear when the alcoholic extract of human bile-pigment is treated with caustic soda, and in gall-stones (as before referred to), sp. 3. It would therefore appear, that in addition to a body more closely resembling *febrile* urobilin, which the fluid contained, an insoluble body was separated which appears to be identical with that kind of urobilin which occurs in bile and in gall-stones.

Action of Caustic Soda and Hydrochloric Acid on Bilirubin.—As there is reason to believe that caustic soda alone changes bilirubin, and that hydrochloric acid oxidises it, I thought it would be interesting to compare the action of these reagents with that of the sodium amalgam. And in order to compare the action of the caustic soda, under the same circumstances as those which may be supposed to occur when sodium amalgam is used, I used a solid piece of pure caustic soda. When a solid piece of caustic soda is thrown into water in which bilirubin is suspended, the fluid becomes orange, showing where it touches the dish a reddish tinge. (After five minutes' action if a little of the fluid be taken out and treated with hydrochloric acid it becomes green in colour.) At the end of twenty minutes the fluid becomes green. If another piece of solid caustic soda be now put in and the fluid examined twenty-three hours after the commencement of the experiment, it is found to be a pale yellow-green colour, showing only general absorption of the violet end of the spectrum. When hydrochloric acid is added it turns red, quickly changing to brown, and giving before the spectroscope a black band, α, wave-length 507 to 482, and a feeble one, δ, 625 to 598. When this brown fluid is treated with caustic soda a shading appears over the violet end beginning at 516. It would therefore appear that the action of the caustic soda and subsequently hydrochloric acid is to oxidise the pigment just beyond the stage of febrile urobilin, and

that it is premature to assume that urobilin is formed from bilirubin by reduction. It would further appear from this experiment, and from a careful study of Gmelin's reaction, that febrile urobilin represents an intermediate stage of oxidation of bile pigment.

Identity of Choletelin and Normal Urobilin.—It will be seen on comparing choletelin with normal urobilin that they cannot be distinguished from each other, being similar in colour, solubility, in spectrum, and in the changes which their respective spectra undergo with reagents. But while choletelin is easily reduced back to febrile urobilin, normal urobilin is *not* easily reduced, because the chemical stability of the latter pigment is greater than that of the former.

On the presence of a body having similar spectroscopic characters to those of Febrile Urobilin in Bile.—When human bile is treated with absolute alcohol to precipitate the mucus, &c., and shaken with chloroform, the latter takes up a good deal of colouring matter, but as a general rule, gives only general absorption of the spectrum. From this fact I had come to the conclusion that the solution could not have contained urobilin, forgetting that the band of that pigment may be invisible when the pigment has been removed from a neutral or slightly alkaline fluid. The following experiments conclusively prove that a body is present in bile which gives the same spectroscopic characters as the body produced by the action of sodium amalgam on bilirubin; and its presence can be proved, not only in the bile of man, but in that of the pig, ox, sheep, and probably in that of all animals possessing a gall-bladder.

Urobilin in Human Bile.—Here I shall principally refer to urobilin, leaving an account of the discovery of hæmatin in bile, until after the production of the urinary pigments from hæmatin has been discussed. I have repeated the following experiments several times, but select one experiment as an illustration of the method adopted for the demonstration of the presence of urobilin. The bile was procured from a case twelve hours after death; the gall-ducts and liver of the subject were free from disease. The bile was treated with absolute alcohol, filtered and shaken with chloroform, the latter separated off after having been allowed to stand some time, and filtered. This solution was orange in colour and gave no band at F. A feeble band could be seen in deep layers covering D, the violet end of the spectrum being shaded by the general absorption characteristic of bilirubin. The chloroform was distilled off over the water-bath, leaving a gamboge-yellow residue, which was extracted with alcohol, which was then filtered, leaving a green-yellow stain on the filtering paper. The residue left after the extraction by the alcohol was an orange powder consisting of almost pure bilirubin.

The alcoholic solution was dark red when examined by transmitted gaslight, and a duller red with transmitted daylight, and with the

latter it was seen to be yellow with a greenish tinge in very thin strata. In deep layers it gave a band covering D, but in shallow layers one at F. (Chart II, sp. 15.) Treated with caustic soda it gave sp. 14,* Chart II, the colour of the fluid becoming light green-yellow. This is the same spectrum as that got by the action of caustic soda on bilirubin treated by sodium amalgam. It will be noticed that the band at F becomes replaced under the action of caustic soda, by another of less intensity of shading and nearer the red end of the spectrum.

Zinc chloride produced a precipitate soluble in alcohol forming a green solution, and then the band at F was seen to be narrowed, and nearer the red; it produced a spectrum in other particulars like that got by the action of caustic soda. Chart II, sp. 16.

Hydrochloric acid produced a turbidity, changing the fluid to dark red, which became clear brown-red with more alcohol; this solution gave sp. 17 in deep layer, while in shallower layer sp. 18 appeared.

Sulphuric acid produced a dark-red fluid, giving almost the same spectrum.

From these observations it was quite evident that the same kind of urobilin was present as that got by the action of sodium amalgam on bilirubin.

Urobilin was absent from the bile in a case of thrombosis of the portal vein, and this observation supports the view that it is formed in the intestine.

Urobilin in the Bile of the Pig.—This bile was golden-yellow, and was treated as in the case of human bile. The chloroformic solution was yellow, and left a chrome-yellow residue, which was entirely soluble in rectified spirit, forming a yellow solution. This gave, in deep layers, general absorption, and in thin strata a band from wave-length 507 to 482.

Zinc chloride produced a precipitate soluble in alcohol, and an abrupt shading commencing at wave-length 510. But in a thinner layer a band was seen detached in the usual position, but owing to the general absorption its violet edge was indistinct. When the alcoholic solution was treated with acetic acid a band, *a*, from wave-length 507 to 478 was visible, the colour of the solution being greenish-yellow.

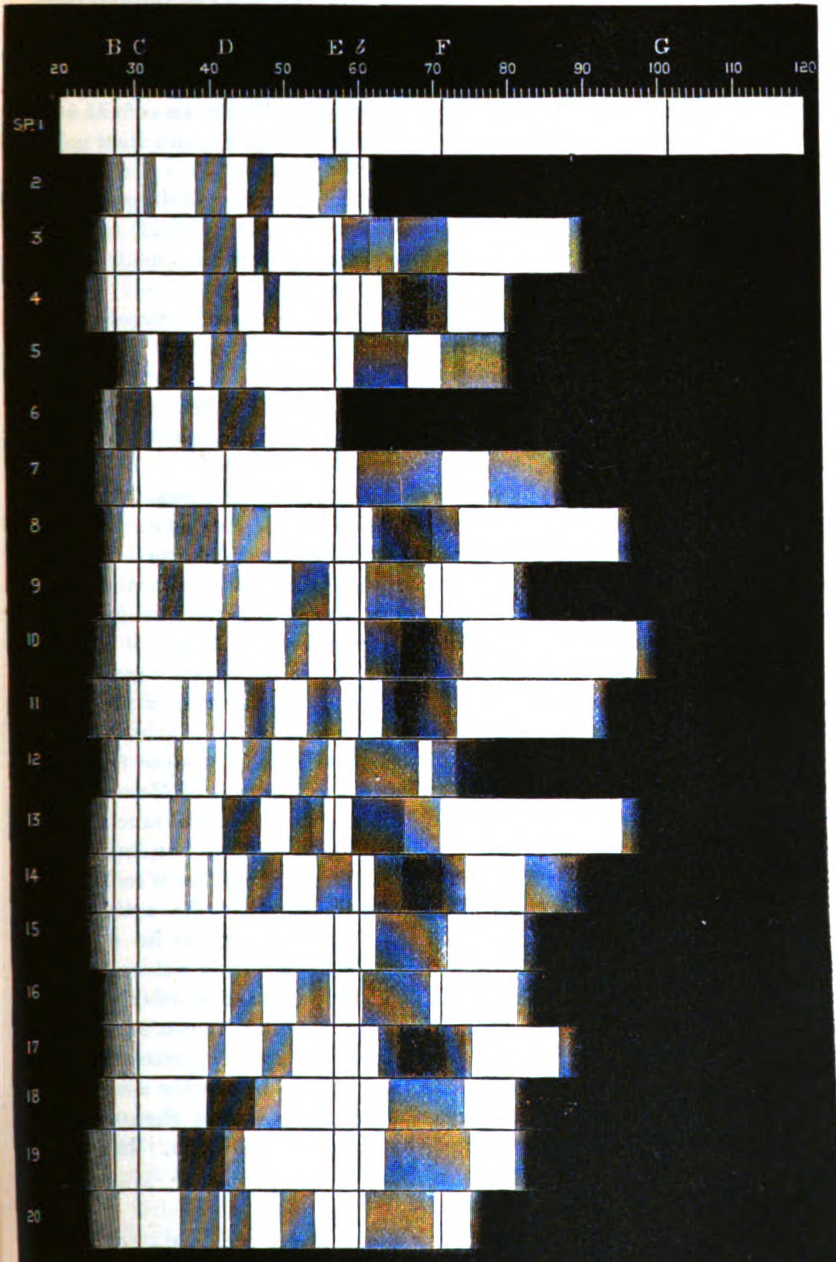
The alcoholic solution treated with caustic soda got pale yellow, and gave the usual band from wave-length 512 to 488.

This observation afforded positive proof of the presence of urobilin in the bile of the pig.

Urobilin in Ox-bile.—This bile was brown in deep, but yellow in shallow strata. It gave sp. 2, Chart III, which is of great interest, as *a similar spectrum can be produced artificially from hæmatin*, to which I

* This map is made to represent two spectra, as both were exactly similar (see explanation of Chart II).

Chart III.


B C D E b F G
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shall again refer. Every possible precaution was taken to exclude the presence of blood. When treated as in former cases, the chloroformic solution was golden-yellow, and gave sp. 3, Chart III. When the chloroform was evaporated, it left a yellow-brown amorphous residue, which was partially soluble in alcohol, forming an orange solution. This gave in a thin stratum sp. 4, Chart III. When this fluid was treated with caustic soda it became light yellow-green, and then gave sp. 5 in a thin layer.*

Chloride of zinc made the alcoholic solution orange-red, giving in deep layer sp. 6, and in shallow sp. 7. When this fluid, already treated by zinc chloride, was treated with hydrochloric acid, sp. 8, Chart III, was seen, the colour being light red.

Ammonia acted in the same manner as caustic soda, except that the band at F could no longer be seen.

Urobilin was, therefore, present in this case.

Urobilin in Sheep-bile.—Its presence can here be demonstrated in the same manner, but as all the spectra are almost identical with those of ox-bile, I will not describe them.

The band at F is affected in all these solutions of human, pig, ox, and sheep bile in exactly the same manner by reagents as in the case of febrile urobilin, but, by the action of ammonia and caustic soda, certain bands in red and orange appear, which, although they are present in urobilin prepared by sodium amalgam from bilirubin, are not always seen in the pigment got from urine. Again, it would appear from numerous observations that, while the biliary pigment is oxidised with comparative ease into choletelin, the urinary pigment requires much stronger oxidising agents to bring about that result.

On the Artificial Production of a Pigment exactly similar to Urohæmatin (excreted in the Urine of Rheumatism) from Acid Hæmatin.—Hoppe-Seyler,† to whom physiological chemists owe so much, was the first who tried the action of tin and hydrochloric acid on hæmatin. He got a pigment which showed such striking resemblance to Maly's hydrobilirubin that he came to the conclusion that the artificially prepared pigment was the same as Maly's pigment, but he noticed that the pigment prepared from hæmatin gave a band before D and one between D and E, as well as α at F. By the action of zinc and sulphuric acid on acid hæmatin or hæmatoin, as it has been named by Professor Preyer, I have succeeded in obtaining a pigment which, when dissolved in various solvents, is found to be exactly similar to the pigment which I isolated from the urine of a case of rheumatism, and which I have taken the liberty of calling urohæmatin. It shows

* Cf. Chart II, sp. 3, 14, and 16, &c.

† "Handbuch der Physiologischen- und Pathologischen- Chemischen Analyse," 4th ed., p. 214, *et seq.*, and "Spectroscope in Medicine," p. 116.

a most remarkable series of bands, in addition to that at F. The experiment has been repeated several times, with an uniform result.

The fresh defibrinated blood of the sheep is treated with alcohol and sulphuric acid (2 parts H_2SO_4 to 35 alcohol) and filtered, more alcohol being afterwards added (if necessary) to help the filtration. This dark-red filtrate gives the spectrum of acid hæmatin, which is seen in sp. 9, Chart III, and which gives the bands of hæmochromogen (reduced hæmatin) with sulphide of ammonium. It is put into a narrow and deep beaker, some fragments of pure zinc and sulphuric acid being added in sufficient quantity to develop a reaction, and a gentle heat is applied to the water-bath over which the beaker is placed. When the action has ceased the fluid is filtered, when it is seen to have become of a much lighter colour. (Sp. 10.*) It is then put into a separating funnel, diluted with water, and shaken with chloroform. The chloroform takes up the pigment, forming a dark-red solution; on separating this off and filtering it, and then distilling the chloroform, a dark-brown pigment is left. It is soluble in alcohol, with a rich colour, and this gives sp. 11, Chart III, in a moderately shallow layer. (Compare 7 and 8, Chart I.)

Ammonia slightly alters the position of the bands, when added to the alcoholic solution, bringing some of them slightly nearer the red end; narrowing and bringing the band at F near the red, but not causing it to disappear. *Caustic soda* produces the same effect as ammonia, sp. 12, Chart III (colour of solution, orange). *Zinc chloride* produces almost the same change, sp. 13, Chart III. (Cf. action on urobilin.) The chloroformic solution of this pigment gives sp. 14, Chart III.

On comparing these spectra with those of the urinary pigment, urohæmatin, it is seen that they are identical band for band, and the description of the reactions with other reagents given by that pigment will apply exactly to this one.

On looking at Preyer's† map of "iron-free hæmatin" a likeness to the present pigment is noticed, but they are different bodies, and the action of sulphuric acid aided by heat had nothing to do with the result; for we can not only prove that a different pigment is produced under those circumstances, but urohæmatin can be prepared in a different manner, in which the influence of sulphuric acid aided by heat is completely excluded.

If the solution of hæmatoin be prepared as before, and it is then shaken with chloroform and water in a separating funnel, the chloroform will take up the hæmatoin, for which I find it is a perfect solvent, and again leave it, after it has been distilled off, in a neutral state.

* Cf. action of sulphuric acid on the natural pigment, Chart I, 12. It is probably these bands that Hoppe-Seyler mentions.

† "Die Blutkrystalle," Tafel I, 15.

If this hæmatoin is then dissolved in alcohol and diluted with water, the solution put into a narrow and deep beaker (or better in a flask), a piece of sodium amalgam added, and the whole gently heated on the water-bath, a change will be found to have taken place after some time. The colour gets much lighter, becoming at last yellow, and it then gives sp. 12, Chart III; this is seen to be the spectrum of urohæmatin prepared by the former method and treated in alcoholic solution with caustic soda. The fluid may then be filtered, the filtrate treated with sulphuric acid to acidity, when its yellow colour changes to orange-red. It is then (after acidification) filtered, put into a separating funnel and shaken with chloroform; this chloroformic solution is reddish and gives sp. 14, Chart III.* When the chloroform is distilled off a dark-brown pigment is left, which alcohol dissolves, forming a red fluid giving sp. 11, Chart III. Treated with caustic soda it gets orange (*i.e.*, less red) and gives sp. 12, Chart III. Ammonia produced the same effect, and its behaviour with other reagents shows that this is the same pigment as that obtained by the action of zinc and sulphuric acid on hæmatoin.

It is, therefore, certain that by the action of reducing agents on hæmatoin a pigment can be prepared, identical with a pigment which can be obtained from urine in certain diseased conditions, and the name urohæmatin best expresses the origin of that pigment.

Artificial production of a Pigment from Acid Hæmatin by Oxidation, indistinguishable from Choletelin and from Normal Urobilin.—The identity of choletelin and urobilin of health has already been proved in this paper. I have now to describe a method by means of which a pigment, which cannot be distinguished from either, can be procured from acid hæmatin (hæmatoin). So far as I know, this experiment has never been described.

A solution of acid hæmatin having been prepared as before, it is treated with peroxide of hydrogen until it changes colour. The red colour first seems to get slightly darker, but it soon changes to brown-yellow; and then a curious change is seen to have taken place in the spectrum. All the bands of acid hæmatin have gone, and instead, a band β, or γ, is seen between green and blue (sp. 15, Chart III), wave-length 507 to 484.

This change can be produced by treating the hæmatoin in the original acid solution, or when separated in the neutral state by means of chloroform, with the peroxide; but if the latter method be adopted we must slightly acidulate again before the pigment can be isolated. The former method is, of course, the easier. If this solution of peroxidised acid hæmatin be put into a funnel and shaken with chloroform, the latter becomes reddish-yellow, and when separated and

* Under certain unknown conditions another feeble band in red may be noticed.

evaporated off, it leaves a *brown-yellow* amorphous pigment. It is soluble in the same solvents as choletelin and normal urobilin. The chloroformic solution appears yellow on a white dish, giving a reddish tint where the fluid touches the white dish, and it gives a band, β or γ, from wave-length 510 to wave-length 484, with ill-defined edges.

Alcohol* dissolved the pigment, forming a yellow solution giving a band, γ, from wave-length 507 to 482. When this yellow fluid was treated with caustic soda it became orange, and gave, in deep layers, general absorption of the violet up to wave-length 534. In shallow layers no band was visible.

Zinc chloride caused the fluid to assume an orange colour, and then the spectrum was shaded up to wave-length 538. In a thinner layer a band became detached (though this was not easily seen) from about wave-length 526 to 501 (?).

When the fluid treated by zinc chloride was treated with caustic soda, the orange-coloured fluid became yellow, and a feeble shadow from wave-length 513 to 488 was just visible.

Re-acidified after treatment with caustic soda a black band came back in the original position. The above characters are sufficient to establish the identity of this pigment with normal urobilin and with choletelin; but its action with sodium amalgam completely proved the truth of this supposition.

Artificial Production of Febrile Urobilin from the Pigment produced by Oxidation of Hæmatoin.—When this brown-yellow pigment is dissolved in alcohol, it gives a yellow solution; when it is diluted with water and sodium amalgam introduced the colour soon becomes orange. After longer action, especially when a gentle heat is applied, the fluid again becomes paler, until at last it assumes that of pale sherry. (See sp. 16, Chart III.) When sulphuric acid is added to acidity, the fluid becomes orange-red, and then three bands are visible, one before D, one between D and E, and a black one at F, sp. 17. But if instead of using strong sulphuric acid, it is added in the proportion of two parts acid to twelve alcohol, and the fluid is then shaken with chloroform, the latter takes up the pigment forming a red solution, which appears yellow in thin layers. On evaporating the chloroform, a *reddish-brown* pigment is left. This dissolves in alcohol with a red colour, and gives in deep layers no bands near D, but in shallower ones a black band at F. It will, therefore, be seen that strong sulphuric acid has the property of so changing the chromogen of the *reduced* pigment, as to produce two bands near D, as well as that at F. I believe that this may account for the presence of certain feeble bands near D, seen in solutions of febrile urobilin. (See "Proc. Roy. Soc," vol. 31, p. 26.)

If to the alcoholic solution of the pigment of a red colour, and

* Ordinary rectified spirit.

giving the black band at F, caustic soda be added, it becomes yellow in colour, and then gives a band, δ, from wave-length 513 to 488. In deep layers two other feeble bands are seen on the violet side of D (Chart III, sp. 16*). When the alcoholic solution is treated with zinc chloride and allowed to stand a few minutes, a narrow band is seen, which is exactly the same band, as regards position and shading, as that seen when solutions of febrile urobilin are treated with zinc chloride.

It would, therefore, appear that by the action of sodium amalgam aided by heat, a colourless, or almost colourless, solution has been obtained, which under the influence of sulphuric acid becomes orange-red, and gives all the characters of febrile urobilin. The original body acted upon with the sodium amalgam being identical with choletelin and with normal urobilin.

On the Action of Decolorised Bile on Hæmoglobin.—Seeing that the urinary pigments, at least such as are recognisable by means of the spectroscope, can be produced with great ease from hæmatin, I was led to think that perhaps hæmatin might be present in the bile, and as a preliminary step to this inquiry I tried the effect of the colourless constituents of the bile on hæmoglobin. Ox-bile was treated with rectified spirit, filtered, then well shaken with animal charcoal in a flask, and again filtered; as this filtrate showed some general absorption of the violet, it was again decolorised. It was then evaporated almost to dryness on the water-bath, and diluted with water; the taste of the solution was exceedingly bitter; it was alkaline, and gave Pettenkofer's reaction and its spectrum. To this fluid 3 cub. centims. of the fresh defibrinated blood of a cat were added. The mixture was put into a hot air-bath, and the bath heated to 110° F; the mixture being stirred with a glass rod from time to time. It soon got darker in colour, and then gave the spectrum of methæmoglobin. The temperature of the bath was then raised to 180°, and the fluid got still darker in colour. After longer action it became a fine crimson, and then gave a band covering D, and one at F, sp. 18, Chart III. After longer action no further change took place. The same body can be produced by the action of caustic soda in alcohol on hæmoglobin.

Action of Caustic Soda in Alcohol on Blood.—When fresh blood (defibrinated) is treated with alcohol and caustic soda, the colour changes to dark red. If the blood so treated contain oxidised hæmoglobin, we get a band at D, and a feebler one at F; but if it contain reduced hæmoglobin, and the reagent is added with exclusion of air, hæmochromogen is formed at the same time. The spectrum got by the action of the reagent on oxidised hæmoglobin is evidently hæmatin,

* Another (*doubtful*) band may have been present in red, its centre at wave-length 625 if so, the reduced pigment was passing by reduction into urohæmatin, with which three of its bands are coincident.

as it gives the bands of hæmochromogen with sulphide of ammonia. A similar pigment may be obtained by isolating hæmatoin as described before, and treating the residue with alcohol and caustic soda, when the same spectrum is seen; but while the pigment obtained directly from oxidised hæmoglobin is easily reduced to hæmochromogen, the latter pigment is reduced with great difficulty.

This alkaline hæmatin, whose spectrum is represented in Chart III, sp. 19,* is easily converted into acid hæmatin again, and thus may be the source of all those kinds of hæmatin from which the biliary and urinary pigments can be formed.

Production of the Spectrum of Sheep-bile from this Pigment.—While by the influence of oxidising agents such as peroxide of hydrogen and permanganate of potassium, this body yields apparently the same pigment as hæmatoin yields, it gives with sodium amalgam in the cold, and with brief action the spectrum of sheep or ox bile, sp. 20,† Chart III. It wants, however, one band in the red, but that also can be made to appear by gentle oxidation with peroxide of hydrogen, and this band in bile only appears after that fluid has been exposed to the air for some time. (Cf. Chart IV, sp. 2, and Chart III, sp. 2.)

When this reduced brownish-red solution was treated with hydrochloric acid, it gave a spectrum very like that got by treating the bile pigments of the sheep with the same reagent;‡ but in order to compare the action of reagents on the respective fluids, it will be necessary to isolate the pigment giving this spectrum from sheep-bile, which has not yet been done. When the solution giving the above spectrum is treated with sulphide of ammonium the bands of hæmochromogen appeared. Consequently, if sheep-bile contains this kind of hæmatin, it should also yield the bands of that substance with sulphide of ammonium.

Hæmatin in the Bile of the Sheep.—Perfectly fresh bile, *which did not contain blood*, and which gave sp. 2, Chart IV, had a few drops of acetic acid added to it, but as this was not sufficient to precipitate the mucus when added in such small quantity, alcohol also was added. The fluid, after filtration, was shaken with chloroform and water. The chloroformic solution, after separation and filtering, had a brown colour, with a slightly greenish tinge, giving sp. 3, Chart IV. The chloroform was evaporated off, leaving an olive-brown residue. This, dissolved in alcohol, formed a green-brown solution, giving sp. 4, Chart IV. When sulphide of ammonium was added to this fluid, the

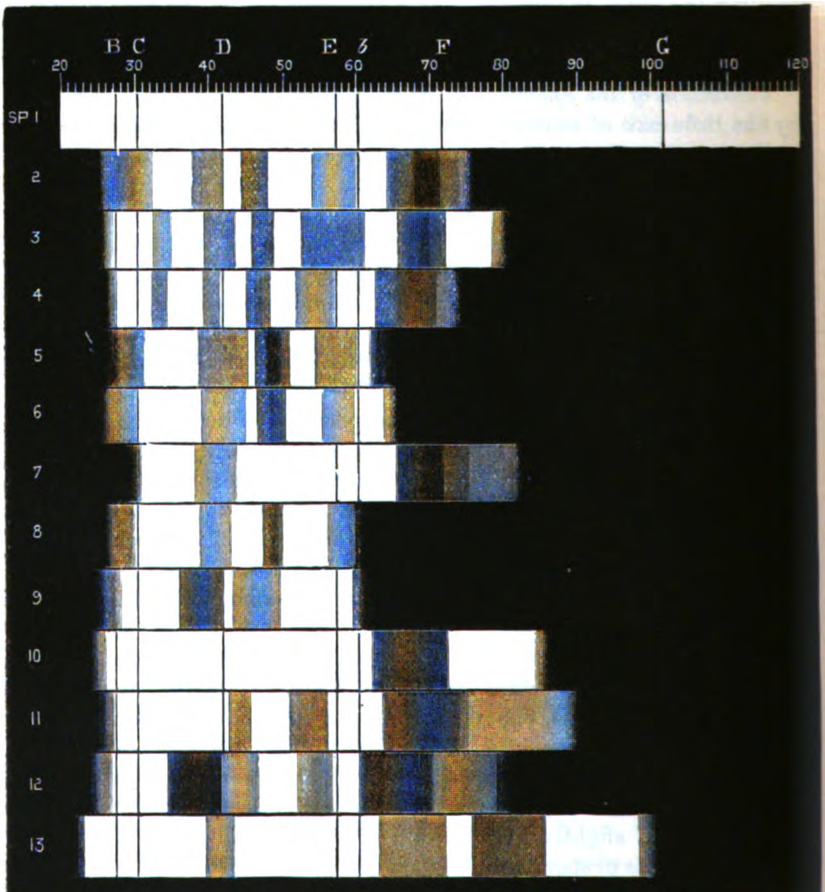
* The difference in position of band near D in 19 and 20 is accounted for when we remember that in one case we are dealing with an alcoholic, and in the other with a syrupy aqueous solution.

† The bands are not coincident with those of 2, because in one case bile, and in the other alcohol, is the solvent for the pigment. (See 2, 3, and 7, Chart IV.)

‡ "Spectroscope in Medicine," Chart II, sp. 9.

bands of hæmochromogen appeared, Chart IV, sp. 5 and sp. 6. This experiment has now been repeated several times, and I believe that the conclusion may be accepted, that sheep-bile contains hæmatin, and similar to that which can be obtained artificially in the manner already described.*

Chart IV.



Hæmatin in Human Bile.—The golden-yellow coloured bile obtained twelve hours after death, from a case of meningitis, and which gave only general absorption, even in thin strata, was treated with a little acetic acid, diluted with water, and shaken with chloroform. The

* The band covering D is not, of course, due to hæmochromogen, but probably to a bile pigment, as referred to before.

residue had a slightly green tinge, and after extraction with alcohol, the alcoholic solution was seen to be a brownish-green colour, and gave the spectrum already described (sp. 7). When this solution was treated with sulphide of ammonium, sp. 8, Chart IV, appeared after it had stood a short time (the original is seen in sp. 7), the colour of the fluid changing to red. Ammonia had nothing to do with this result. It therefore appears certain that human bile also contains hæmatin, but in less quantity than the bile of the sheep.*

If the curious series of bands seen in sheep-bile are due to the presence of hæmatin, it is probable that the bands seen in the red, orange, and green parts of the spectrum characteristic of the spectra of the bile of other animals, are also due to its presence. In the bile of the crow these bands are like those of the bile of the sheep and ox; in that of the guinea-pig the single band may be due to the darker band of hæmochromogen; those of the rabbit appear to be the bands of that substance, and I have no doubt that the presence of hæmatin in the bile of these animals† will be proved as easily as in the case of man and the sheep. Of course I do not include the band at F, as that is always due to the presence of urobilin.

Action of Peroxide of Hydrogen on the Alcoholic Extract of Human Bile Pigments.—When the brown-green alcoholic extract of the chloroformic residue of human bile pigment (see sp. 7, Chart IV), and which was proved to contain hæmatin, urobilin (of biliary origin), and other pigments, was treated by peroxide of hydrogen on a white dish, the colour changed from brown-green to dark-green, blue-green, blue, violet, red, red-brown, and brown-yellow. At the violet stage three bands were visible, sp. 9, Chart IV;‡ at the brown-yellow stage only one, that at F, sp. 10. It was, therefore, evident that the mixture of pigments could be oxidised into choletelin with great ease.

The Absorption Band of Serum.—The band at F in blood-serum has been said to be due to lutein;§ whether that substance is or is not present, there is evidence to show that an *oxidised bile pigment is that which gives the band*, and I have come to the conclusion, from a careful examination of fresh blood-serum, obtained by letting the blood of the sheep clot spontaneously, and filtering the yellow serum, that it contains either choletelin or a substance like it. The serum gave sp. 11, Chart IV, which shows that it still contained traces of hæmoglobin. The band at F γ read from wave-length 504 to 480, it was therefore nearer the red than the band of lutein, and I could

* As proved by the presence of *first* band of reduced hæmatin, the second being just visible and overlapped by general absorption. (See 8, Chart IV.)

† See Chart II, "Spectroscope in Medicine."

‡ That at F will be represented by next spectrum, 10.

§ Maly believed he had detected urobilin in blood, but Hoppe-Seyler thought he had mistaken lutein for it. See "Hand. der Phys. und. Path. Chem. Anal.," *loc. cit.*

not, by any method, see the second band of lutein in violet. Moreover, caustic soda and ammonia which intensify the band of lutein, *caused this band to disappear;* and when zinc chloride in very small quantity was added after the caustic soda a precipitate fell, but when this was separated from the fluid, I thought I could perceive a faint band from wave-length 516 to 488. If the white of egg is compared with this, as I find that it contains some of the lutein of the yelk, we see two bands distinctly, and that nearer the red is decidedly *darkened* by both caustic soda and by ammonia; moreover the first lutein band of the yelk in alcohol read from wave-length 496 to 478. (Lutein itself, even if present, may also be formed in the liver, as I have found it in gall-stones (*vide antea*), and a pigment giving its spectrum appears to be formed by the long-continued action of caustic soda on the alcoholic extract of the pigments of sheep bile, sp. 4. Thus, the first action was to give sp. 12, Chart IV, but after half-an-hour sp. 13 appeared, the other bands having faded away.)

I therefore conclude that the absorption-band of serum is due to a body which is produced by the oxidation of the bile pigments, and which is on its way to be excreted by the kidneys.*

Summary and Conclusions.

(1.) That normal human urine contains a body as such, which is apparently identical with choletelin and with the body produced by the action of peroxide of hydrogen on acid hæmatin.

(2.) That normal human urine contains the chromogen of febrile urobilin, which can be prepared artificially by reduction of choletelin, and of the body produced by oxidation from hæmatoin.

(3.) That human, ox, sheep, and pig bile contain a kind of urobilin, which differs in some respects from that excreted in urine, and that they also contain hæmatin.

(4.) That it is highly probable that all the constituents of bile colouring matter are produced from hæmatin by reduction.

(5.) That the hæmatin present in bile is probably due to the action of the bile acids on hæmoglobin.

(6.) That all the colouring matters of bile, including hæmatin, urobilin of biliary origin, bilirubin, &c., are oxidised into choletelin, and that there is evidence to show that blood-serum contains this body, which is on its way to be excreted by the kidneys.

(7.) That the absorption-bands seen in the bile of various animals are due to the presence of hæmatin and urobilin of biliary origin.

(8.) That a pigment excreted in the urine in certain pathological conditions is derived from hæmatin by reduction, as it can be obtained by reducing acid hæmatin with zinc and sulphuric acid, and also by

* Neubauer and Vogel ("Guide to Analysis of Urine," American edition, 1879, p. 64) think this band is due to urobilin.

means of sodium amalgam; that it is not febrile urobilin, and as it is derived directly from hæmatin it is best named urohæmatin.

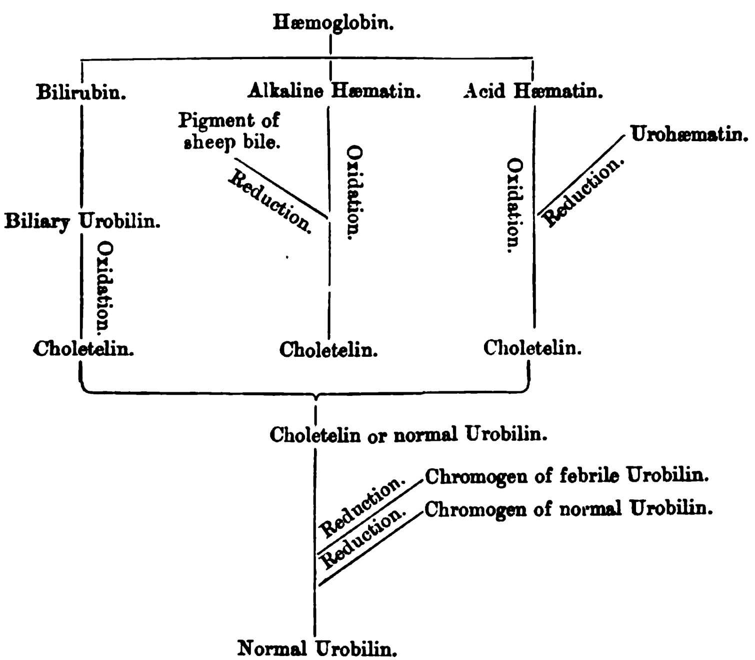
(9.) That the urobilin of bile is produced in the intestine.

(10.) That the urobilin of bile may, in certain states of the system, appear in the urine, but that under normal conditions it is oxidised into choletelin in common with the other biliary pigments, and comes to the kidneys as choletelin, while a part may pass into the urine as such, but a part becomes reduced in the kidneys into the chromogen of febrile, and perhaps also into the chromogen of normal urobilin; the former by strong oxidising agents passing into febrile urobilin, and the latter by the action of weaker oxidising agents into normal urobilin.

(11.) That many of the colouring matters of urine have been produced, by the action of the reagents designed to separate them, on these chromogens.

(12.) While most of the urinary pigments are traceable back to the bile pigments,* there is evidence to show that some of them are derived from hæmatin directly, and pigments derived from that source may entirely replace the normal pigments.

A diagram will clearly explain the connexion, which appears from this research to exist between all these pigments:—



* Bilirubin, biliverdin, &c.

There appears to be one point of difference between normal urobilin and choletelin and the pigment obtained from hæmatoin: that is, that while chloride of zinc produces a narrow band nearer the red with an alcoholic solution of normal urobilin, it does not produce that band with the latter pigments until they have been slightly reduced with sodium amalgam; this would show that some change (reduction) has taken place in the normal urinary pigment during its passage from the blood into the urine.

EXPLANATION OF CHARTS.

The accompanying charts were mapped from the microspectroscope provided with an accurately divided photographed scale, and the scale at the top of each chart is that of the instrument. Accompanying these charts is a table giving the value of each division of the scale in wave-lengths. I have adopted this plan in order to avoid having two scales adapted to the charts; and a scale of wave-lengths would not be accurate adapted to maps drawn this size.

CHART I.

Spectrum 1. Solar spectrum.
" 2. Normal urobilin in alcohol.
" 3. The same treated with chloride of zinc.
" 4. Action of sodium amalgam on brown-yellow normal urobilin, and of caustic soda on brown urobilin.
" 5. Acidulated alcoholic extract of urohæmatin from urine of rheumatism; deep layer.
" 6. Chloroformic solution of the same pigment.
" 7. Alcoholic solution of ditto.
" 8. Shallow depth of the same.
" 9. Deep layer of alcoholic solution (7) treated with caustic soda or ammonia.
" 10. The same, shallow depth.
" 11. Urohæmatin in alcohol, treated with hydrochloric acid.
" 12. The same treated with sulphuric acid.
" 13. The same with sodium amalgam.
" 14. The pigment (acted on by amalgam) with hydrochloric acid.
" 15. Acidulated alcohol extract of pigment got from the urine of a case of pleurisy; deep layer.
" 16. Ditto, shallow layer.
" 17 and 18. The alcoholic solution of this pigment treated with caustic soda, in deep and shallow layers.

CHART II.

Spectrum 1. Solar spectrum.
" 2. Alcoholic extract human gall-stone.
" 3. The same treated with caustic soda or ammonia.
" 4. Chloroformic solution of bilirubin.
" 5, 6, 7, 8, 9, 10. Action of chlorine on this solution.
, 11. The pigment of stage represented in sp. 10 treated with sodium amalgam and afterwards hydrochloric acid.

Spectrum 12. Pigment got by action of sodium amalgam on bilirubin dissolved in alcohol, and precipitated by means of hydrochloric acid.*
„ 13. The same, thin layer (the band at F is shown rather too dark.)
„ 14. The same treated with caustic soda.†
„ 15. Alcoholic solution of chloroformic residue of human bile pigments, deep layer; in a thinner layer another band is seen wave-length 504 to 480.
„ 16. Ditto, treated with zinc chloride.
„ 17. Ditto, treated with hydrochloric acid.
„ 18. Thin layer of the same.

Compare 2, 7, 12, and 17, and 3, 14, and 16.

Chart III.

Spectrum 1. Solar spectrum.
„ 2. Ox-bile.
„ 3. Chloroformic solution pigments of ox-bile. (Compare 2 and 3 with 20, *infra.*)
„ 4. Pigments of ox-bile in alcohol.
„ 5. The same treated with caustic soda.
„ 6. Action of chloride of zinc on alcoholic solution (4).
„ 7. The same, shallow depth.
„ 8. The same (7) treated with hydrochloric acid.
„ 9. Hæmatoin (acid hæmatin) in alcohol.
„ 10. Hæmatoin treated with zinc and sulphuric acid and filtered. (Compare Chart 1, 11, 12, 14.)
„ 11. The isolated urohæmatin in alcohol.
„ 12. The alcoholic solution with caustic soda.
„ 13. The alcoholic solution with zinc chloride.
„ 14. The artificially prepared urohæmatin in chloroform.
„ 15. Action of peroxide of hydrogen on hæmatoin.
„ 16. The pigment produced by oxidation isolated and reduced by sodium amalgam and filtered.
„ 17. This filtrate treated with sulphuric acid.
„ 18. Spectrum produced by the action of decolorised bile on hæmoglobin.
„ 19. The same or a similar spectrum, got by acting on blood with alcohol and caustic soda.
„ 20. Spectrum closely resembling sheep or ox bile, produced by the action of sodium amalgam for half-an-hour in the cold, on the body whose spectrum is shown in 19. (Compare 2 and 3, *supra.*)

Chart IV.

Spectrum 1. Solar spectrum.
„ 2. Sheep-bile.
„ 3. Pigments of sheep-bile in chloroform.
„ 4. Pigments of sheep-bile in alcohol.
„ 5. Bands of hæmochromogen, got by adding sulphide of ammonium to fluid giving 4.

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* And dissolved in alcohol.

† This also represents solution of which sp. 15 is the map treated with caustic soda.

Spectrum 6. Thinner stratum of the same.
„ 7. Alcoholic extract of human bile pigments.
„ 8. The same treated with sulphide of ammonium, showing the presence of hæmochromogen.
„ 9. Alcoholic extract of human bile pigments with peroxide of hydrogen, violet stage.
„ 10. The same—end of reaction.
„ 11. The absorption-band of serum, from blood of sheep (showing also O-hæmoglobin bands).
„ 12. Spectrum got by treating alcoholic extract of sheep-bile pigments with caustic soda for a short time.
„ 13. Spectrum closely resembling lutein got by longer action. *This spectrum was also produced from hæmatin reduced by sodium amalgam, or at all events a spectrum very like it, by somewhat similar treatment.*

Table giving the wave-length corresponding to each division of the scale in millionths of a millimetre.

Scale reading.	Wave-length.	Scale reading.	Wave-length.
28	683	56	530
29	671	57	526
30	660	58	523
31	654	59	520
32	646	60	516
33	639	61	513
34	632	62	510
35	625	63	507
36	620	64	504
37	614	65	501
38	608	66	499
39	603	67	496
40	598	68	493
41	593	69	491
42	588	70	488
43	584	71	486
44	580	72	484
45	576	73	482
46	571	74	480
47	567	75	478
48	562	76	476
49	558	77	474
50	555	78	472
51	550	79	469
52	546	80	467
53	542	81	465
54	538	82	463
55	534	83	461

Scale reading.	Wave-length.	Scale reading.	Wave-length.
84	459	94	442
85	457	95	440
86	455	96	438
87	453	97	437
88	451	98	435
89	450	99	434
90	448	100	432
91	447	101	430
92	445	102	429
93	443	103	427·5

IV. "Note on the Determination of Magnetic Inclination in the Azores." By T. E. THORPE, Ph.D., F.R.S. Received November 13, 1880.

With the exception of a series of determinations made by the officers of the "Challenger" at Ponta Delgada, St. Michael, in 1873, no magnetic observations have, so far as I can learn, been made in the Azores since the time of Captain Vidal's hydrographic survey in 1843–4. A visit to these islands during the past summer has enabled me to offer the small contribution to their magnetic history which forms the subject of the present communication.

Magnetic observations are made with some difficulty in the Azores, on account of the intensely volcanic character of the islands. Considerable care was however taken in selecting the stations, and there is no reason to suppose that the observations are affected to any great extent by the nature of the soil or rock immediately beneath the instrument or in proximity to it. The places chosen were such as will enable subsequent observers to repeat the determinations on the same spots.

The dip-circle employed was Dover 3, belonging to the Owens College, Manchester: I had previously used this instrument in the course of my magnetic observations along the fortieth parallel in North America ("Proc. Roy. Soc.," vol. 30, p. 132), and am again indebted to Professor Balfour Stewart for the loan of it. It was provided with two needles, each $3\frac{1}{2}$ inches long and 0·27 inch in maximum breadth. The same precautions were taken to preserve the needles from rust as are described in the communication above referred to, and the method of observation was identical with that previously adopted. In all cases duplicate and independent observations were made with the two needles.

The results are as follow :—

I. Island of St. Michael.

In the garden of Senhor José do Canto, Santa Anna, Ponta Delgada. Approximate position: lat. 37° 45′ N., long. 25° 40′ W.

The spot was that selected for the "Challenger" observations: Mr. do Canto has now marked it by a small (stone?) pillar.

	Local time.	Needle 1.	Needle 2.	Mean.
Aug. 23, 1880.	12.0 to 3.30 P.M.	62° 40′·0	62° 40′·5	62° 40′·2 N.

The "Challenger" observations in 1873 gave 63° 56′·8 N.*

II. Island of Terceira.

At Angra: in the ground near the monument to Dom Pedro IV, 8 paces to the north of the Obelisk. Approximate position: lat. 38° 39′ N., long. 27° 14′ W.

	Local time.	Needle 1.	Needle 2.	Mean.
Sept. 16, 1880.	11 A.M. to 12.12 P.M.	64° 10′·5	64° 10′·1	64° 10′·3

III. Island of Fayal.

At Horta: 32 paces E.N.E. (magnetic) to the front of the Clock Tower to the north of the town. Approximate position: lat. 38° 32′ N., long. 28° 38′ 30 W.

		Needle 1.	Needle 2.	Mean.
Sept. 1, 1880.	12.30 to 1.40 P.M.	63° 38′·5	63° 38′·5	63° 38′·5

Before leaving Lisbon for the Azores I made a set of observations near Moita, at the Quinta do Esteiro Furado, belonging to Mr. T. Creswell, which it may be desirable to include here. The station was identical with that on which I made the series of determinations of Photo-chemical Intensity, published in the "Philosophical Transactions" for 1870. ("On the Relations between the Sun's Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky," Roscoe and Thorpe, "Phil. Trans.," 1870, p. 309.) The position was, approximately, lat. 38° 40′ N., long. 9° W. Hence it is practically in the same parallel as the Azores.

		Needle 1.	Needle 2.	Mean.
Aug. 3, 1880.	10.30 A.M. to 12.45 P.M.	59° 03′·0	59° 03′·5	59° 03′·2 N.

* Communicated by Captain Evans, through Mr. Whipple.

V. "On Heat Conduction in Highly Rarefied Air." By William Crookes, F.R.S. Received November 18, 1880.

The transfer of heat across air of different densities has been examined by various experimentalists, the general result being that heat conduction is almost independent of pressure. Winkelmann ("Pogg. Ann.," 1875–76) measured the velocity of cooling of a thermometer in a vessel filled with the gas to be examined. The difficulty of these experiments lies in the circumstance that the cooling is caused not only by the conduction of the gas which surrounds the cooling body, but that also the currents of the gas and, above all, radiation play an important part. Winkelmann eliminated the action of currents by altering the pressure of the gas between 760 and 1 millim. (with decreasing pressure the action of gas currents becomes less), and he obtained data for eliminating the action of radiation by varying the dimensions of the outer vessel. He found that, whereas a lowering of the pressure from 760 to 91·4 millims. there was a change of only 1·4 per cent. in the value for the velocity of cooling, on further diminution of the pressure to 4·7 millims. there was a further decrease of 11 per cent., and this decrease continued when the pressure was further lowered to 1·92 millim.

About the same time Kundt and Warburg ("Pogg. Ann.," 1874, 5) carried out similar experiments, increasing the exhaustion to much higher points, but without giving measurements of the pressure below 1 millim. They enclosed a thermometer in a glass bulb connected with a mercury pump, and heated it to a higher temperature than the highest point at which observations were to be taken; then left it to itself, and noted the time it took to fall through a certain number of degrees. They found that between 10 millims. and 1 millim. the time of cooling from 60° to 20° was independent of the pressure; on the contrary, at 150 millims. pressure the rate was one-and-a-half times as great as at 750 millims. Many precautions were taken to secure accuracy, but no measurements of higher exhaustions being given the results lack quantitative value.

It appears, therefore, that a thermometer cools slower in a so-called vacuum than in air of atmospheric pressure. In dense air convection currents have a considerable share in the action, but the law of cooling in vacua so high that we may neglect convection, has not to my knowledge been determined. Some years ago Professor Stokes suggested to me to examine this point, but finding that Kundt and Warburg were working in the same direction it was not thought worth going over the same ground, and the experiments were only

tried up to a certain point, and then set aside. The data which these experiments would have given are now required for the discussion of some results on the viscosity of gases, which I hope to lay before the Society in the course of a few weeks; I have therefore completed them so as to embody the results in the form of a short paper.

An accurate thermometer with pretty open scale was enclosed in a $1\frac{1}{2}$ inch glass globe, the bulb of the thermometer being in the centre, and the stem being enclosed in the tube leading from the glass globe to the pump.

Experiments were tried in two ways:—

I. The glass globe (at the various exhaustions) was immersed in nearly boiling water, and when the temperature was stationary it was taken out, wiped dry, and allowed to cool in the air, the number of seconds occupied for each sink of 5° being noted.

II. The globe was first brought to a uniform temperature in a vessel of water at 25°, and was then suddenly plunged into a large vessel of water at 65°. The bulk of hot water was such that the temperature remained sensibly the same during the continuance of each experiment. The number of seconds required for the thermometer to rise from 25° to 50° was registered as in the first case.

It was found that the second form of experiment gave the most uniform results; the method by cooling being less accurate, owing to currents of air in the room, &c.

The results are embodied in the following table:—

(Rate of Heating from 25° to 50°.)

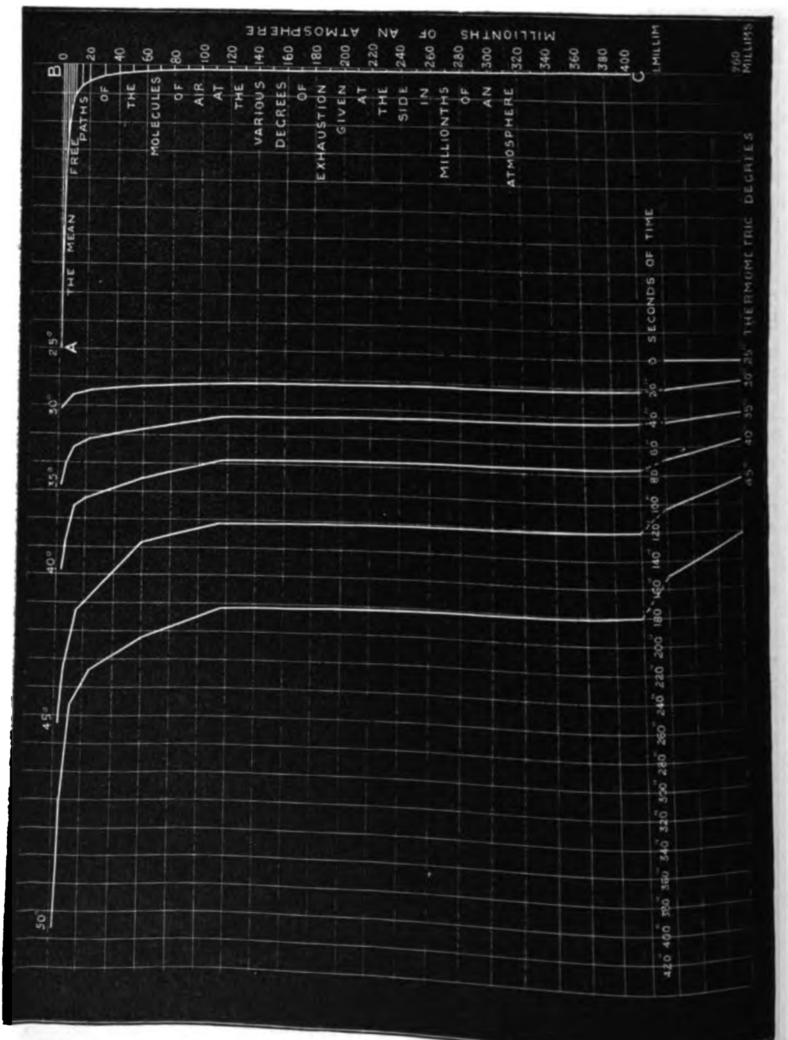
Table I.

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
760 millims. ..	25° ..	0 ..	0
	25 to 30 ..	15 ..	15
	30 35 ..	18 ..	33
	35 40 ..	22 ..	55
	40 45 ..	27 ..	82
	45 50 ..	39 ..	121
1 millim. ..	25° ..	0 ..	0
	25 to 30 ..	20 ..	20
	30 35 ..	23 ..	43
	35 40 ..	25 ..	68
	40 45 ..	34 ..	102
	45 50 ..	48 ..	150

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
620 M.*	25°	0	0
	25 to 30	20	20
	30 35	23	43
	35 40	29	72
	40 45	37	109
	45 50	33	162
117 M.	25°	0	0
	25 to 30	23	23
	30 35	23	46
	35 40	32	78
	40 45	44	122
	45 50	61	183
59 M.	25°	0	0
	25 to 30	25	25
	30 35	30	55
	35 40	36	91
	40 45	45	136
	45 50	67	203
23 M.	25°	0	0
	25 to 30	28	28
	30 35	33	61
	35 40	41	102
	40 45	55	157
	45 50	70	227
12 M.	25°	0	0
	25 to 30	30	30
	30 35	37	67
	35 40	41	108
	40 45	58	166
	45 50	86	252
5 M.	25°	0	0
	25 to 30	38	38
	30 35	43	81
	35 40	54	135
	40 45	71	206
	45 50	116	322

* M = millionth of an atmosphere.

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
2 M.	25°	0	0
	25 to 30	41	41
	30 35	51	92
	35 40	65	157
	40 45	90	247
	45 50	165	412



I have embodied these results in the preceding diagram. The ordinates represent the number of seconds occupied during the rise of each 5°, starting from 25°; the abscissæ represent the pressure. The lower portion gives the total variation in time between pressures of 760 millims. and 1 millim. The upper and larger portion of the diagram gives the abscissæ in millionths of an atmosphere. At the right side of the diagram, in the space A B C, I have drawn a series of horizontal lines increasing in length from 0·25 millim. at 400 M., to 100 millims. at 1 M. These show the actual lengths of the mean free path of the molecules of air at the degrees of exhaustion to which they are opposite.* The parallelism between the curves formed by joining the ends of these horizontal lines and the curves representing the rate of cooling is sufficiently close to justify the inference that they are associated phenomena.

There are two ways in which heat can get from the glass globe to the thermometer—(1) By radiation across the intervening space; (2) by communicating an increase of motion to the molecules of the gas, which carry it to the thermometer. It is quite conceivable that a considerable part, especially in the case of heat of low refrangibility, may be transferred by "carriage," as I will call it to distinguish it from convection which is different, and yet that we should not perceive much diminution of transference, and consequently much diminution of rate of rise with increased exhaustion, so long as we work with ordinary exhaustions up to 1 millim. or so. For if, on the one hand, there are fewer molecules impinging on the warm body (which is adverse to the carriage of heat), yet on the other the mean length of path between collisions is increased, so that the augmented motion is carried further. The number of steps by which the temperature passes from the warmer to the cooler body is diminished, and accordingly the value of each step is increased. Hence the increase in the difference of velocity before and after impact may make up for the diminution in the number of molecules impinging. It is therefore conceivable that it may not be till such high exhaustions are reached that the mean length of path between collisions becomes comparable with the diameter of the case, that further exhaustion produces a notable fall in the rate at which heat is conveyed from the case to the thermometer.

The above experiments show there *is* a notable fall, a reduction of pressure from 5 M. to 2 M., producing twice as much fall in the rate as is obtained by the whole exhaustion from 760 millims. to 1 millim. We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow.

* In the published diagram the lengths have been reduced by the engraver in the proportion of 8 to 3.

VI. "On the Thermo-Electric Behaviour of Aqueous Solutions with Platinum Electrodes." By G. Gore, LL.D., F.R.S. Received November 13, 1880.

The object of the following research was to enable a comparison to be made of the results obtained in a previous investigation with mercurial electrodes (see "On the Thermo-Electric Behaviour of Aqueous Solutions with Mercurial Electrodes," "Proc. Roy. Soc.," vol. 29) with those of platinum in similar liquids, in order to ascertain whether the results further confirmed the conclusion that the currents obtained with mercury were not at all due to chemical action; and also to draw from the results such additional inferences as were warranted. The results of a former research, in which platinum electrodes were employed in a different apparatus (see "Proc. Roy. Soc.," vol. 27) could not be used for the purpose of comparison, because it was found necessary to use weaker solutions with mercurial electrodes (in order to avoid chemical action) than those used with platinum in that research.

The apparatus employed in the present investigation was precisely similar to that used in the one with mercury, with the exception that coiled ribbons of sheet platinum (with attached wires of that metal) were substituted for the layers of mercury.

The platinum ribbons were 27·5 millims. wide, and each had a total surface of 52,960 sq. millims., including both sides. They were prepared for use at the outset by heating them to redness. Between each experiment it was found necessary to thoroughly wash them, then immerse them in boiling distilled water during about ten or fifteen minutes, drain and dry them. They were then simultaneously immersed in the liquid to be tested, the latter being at a temperature of about 55° F. A slight temporary deflection of the needles of the galvanometer was usually produced on immersion; when the needles had settled at zero, heat was applied to one of the vessels, and its temperature raised to 180° F., the deflection being noted. After the experiment had been made, the two portions of liquid were mixed and cooled.

As this research was made chiefly for the purpose of comparing the thermo-electric behaviour of liquids with mercury to that with platinum, precisely similar liquids were employed to those used in the investigation referred to. None of the solutions acted chemically (in any perceptible degree) upon platinum. It was found necessary to preboil the solution about half-an-hour, in order to expel dissolved air; otherwise the deflections obtained in repetition experiments were not uniform. It was also found necessary to occasionally immerse the ribbons in a boiling-hot dilute solution of pure potassic hydrate, during

about a quarter of an hour, to remove impurities caused by handling. Repetition experiments were made with each liquid, and, under the above conditions, the results obtained were usually very uniform, the variation of amount of deflection of the needles not often exceeding half a degree. The galvanometer employed was the same as in the research referred to, and was used in the same manner.

The following are the results obtained with different liquids.

Behaviour of Different Solutions.

Experiment No. 1. Twelve ounces of water and 3 grs. of potassic cyanide of a high degree of purity. Hot platinum was positive 1° at 180° F. (Note *a.*)

No. 2. A solution (note *b*) of 6 grs. of the same salt. Hot platinum positive 2°.

No. 3. A solution of 60 grs. of the same salt. Hot platinum positive $8\frac{1}{4}$°.

Note a. All the deflections, except those otherwise stated, were taken at the temperature of 180° F.

Note b. Unless otherwise described, 12 ozs. of water were used in every case to form the solution, and each solution was made of similar strength to the corresponding one in the research with mercurial electrodes.

No. 4. A solution of 120 grs. of the same salt. Hot platinum positive 11°.

No. 5. A solution of 12 minims of selenic acid (of sp. gr. 1·32 at 60° F.). Cold platinum positive $\frac{3}{4}$°.

No. 6. A solution of 24 minims of the same acid. Cold platinum positive 1°.

No. 7. A solution of 30 grs. of anhydrous selenate of sodium. Hot platinum positive $\frac{3}{4}$°.

No. 8. A solution of 120 grs. of crystalline sulphate of ammonium. Hot platinum positive $3\frac{1}{4}$°.

No. 9. A solution of 60 grs. of crystalline ammonic alum. Cold platinum positive $\frac{3}{4}$°.

No. 10. A solution of 120 grs. of the same salt. Cold platinum positive 1°.

No. 11. A solution of 48 minims of formic acid of sp. gr. 1·18. Hot platinum positive 1°.

No. 12. A solution of 96 minims of the same acid. Hot platinum positive $1\frac{1}{2}$°.

No. 13. A solution of 120 grs. of potassic nitrate. Hot platinum positive 1°.

No. 14. A solution of 120 grs. of crystalline potassic sulphate. Hot platinum positive 4°.

No. 15. A mixture of seventy-nine volumes of water, and one of pure sulphuric acid. Cold platinum positive $3\frac{1}{4}$°.

No. 16. A mixture of thirty-nine volumes of water, and one of the same acid. Cold platinum positive $4\frac{1}{4}°$.

No. 17. A mixture of nineteen volumes of water, and one of the same acid. Cold platinum positive $3\frac{3}{4}°$.

No. 18. A solution of 60 grs. of pure and partly dehydrated sodic carbonate. Hot platinum positive $5\frac{1}{2}°$.

No. 19. A solution of pure and partly dehydrated potassic carbonate. Hot platinum positive 8°.

No. 20. A solution of 120 grs. of crystalline sodic sulphate. Hot platinum positive $\frac{3}{4}°$.

No. 21. A solution of 120 grs. of ammonic nitrate. Hot platinum positive $\frac{3}{4}°$.

No. 22. A solution of 120 grs. of crystalline boracic acid. Hot platinum faintly positive.

No. 23. A solution of 120 grs. of sodic nitrate. Hot platinum positive $\frac{1}{2}°$.

No. 24. A solution of 120 grs. of crystalline phosphate of sodium. Hot platinum positive 3°.

No. 25. A solution of 60 grs. of baric nitrate. Hot platinum positive $\frac{1}{2}°$.

No. 26. A solution of 60 grs. of crystalline nitrate of strontium. Hot platinum positive 1°.

No. 27. A solution of 120 grs. of crystalline phosphate of ammonium. Hot platinum positive 6°.

No. 28. A solution of 60 grs. of crystals of borax. Hot platinum positive 2°.

No. 29. A solution of 60 grs. of crystalline sodic hyposulphite. Hot platinum positive $5\frac{1}{4}°$.

No. 30. A solution of 12 grs. of crystals of sodic sulphite. Hot platinum positive 1°.

No. 31. A solution of 60 grs. of the same salt. Hot platinum positive $3\frac{1}{2}°$.

No. 32. A solution of 60 grs. of potassic sulphite. Hot platinum positive 5°.

No. 33. A solution of 60 grs. of crystalline phosphate of sodium and ammonium. Hot platinum positive $\frac{3}{4}°$.

No. 34. A solution of 120 grs. of the same salt. Hot platinum positive 1°.

No. 35. A solution of 60 grs. of formiate of sodium. Hot platinum positive 4°.

No. 36. A solution of 120 grs. of the same salt. Hot platinum positive 5°.

In a former research ("Proc. Roy. Soc.," vol. 27, No. 188, p. 513, 1878) with platinum electrodes, in which a different form of apparatus was employed, but in which the solutions used were very much more concentrated, the direction of the current was not affected by the

difference of strength of the liquids, except in the instance of Ammonic alum; in that case the neutral point occurred at a lower temperature with the stronger solution.

The following is the order obtained by arranging the above results in a series, with the solution at the top in which hot platinum was most positive at 180° F., and that at the bottom in which it was most negative. The liquids above the horizontal line are those in which the hot metal was positive, below it are placed those in which it was negative:—

1.	Cyanide of potassium (120 grs.)	11·0 +
2.	Cyanide of potassium (60 grs.)	8·25
3.	Carbonate of potassium	8·0
4.	Phosphate of ammonium	6·0
5.	Carbonate of sodium	5·5
6.	Hyposulphite of sodium	5·25
7.	Formiate of sodium (120 grs.)	5·0
8.	Sulphite of potassium	5·0
9.	Sulphate of potassium	4·0
10.	Formiate of sodium (60 grs.)	4·0
11.	Sulphite of sodium (60 grs.)	3·5
12.	Sulphate of ammonium	3·5
13.	Phosphate of sodium	3·0
14.	Borax	2·0
15.	Cyanide of potassium (6 grs.)	2·0
16.	Formic acid (96 minims)	1·5
17.	Microcosmic salt (120 grs.)	1·0
18.	Nitrate of potassium	1·0
19.	Nitrate of strontium	1·0
20.	Cyanide of potassium (3 grs.)	1·0
21.	Sulphite of sodium (12 grs.)	1·0
22.	Formic acid (48 minims)	1·0
23.	Nitrate of ammonium	·75
24.	Microcosmic salt (60 grs.)	·75
25.	Sulphate of sodium	·75
26.	Selenate of sodium	·75
27.	Nitrate of sodium	·5
28.	Nitrate of barium	·5
29.	Boracic acid	faintly positive.

30.	Ammonia alum (60 grs.)	·75
31.	Selenic acid (12 minims)	·75
32.	Selenic acid (24 minims)	1·0
33.	Ammonia alum (120 grs.)	1·0
34.	Sulphuric acid (1 in 80)	3·25
35.	Sulphuric acid (1 in 20)	3·75
36.	Sulphuric acid (1 in 40)	4·25

In order to arrange these solutions in a series, according to their various degrees of electromotive force, a similar plan was adopted to that employed when liquids of the same composition were used with mercurial electrodes; viz., two exactly similar apparatuses, with electrodes of the same dimensions, were employed, one being charged with one solution, and the other with the solution next to it in the above series. The apparatuses were connected with the wires of the differential galvanometer in such a manner that the two currents passed through the two coils in opposite directions; the direction and amount of deflection being noted when the liquid in one vessel of each apparatus was at 160° F. The series thus obtained is so arranged that the couple giving the strongest positive current is at the top, and that giving the most powerful negative current at the bottom, as in the list already given. The solutions in which hot platinum was positive are separated from those in which it was negative by a horizontal line as in the above series.

The positions of the solutions in the two series, although they agree very closely, are not identical; for instance, the two solutions of formiate of sodium occupy respectively the seventh and tenth places in the first series, and the third and eighth in the second series; the positions of a few other solutions are also changed, but not to so great an extent. It will be observed that these two series agree more closely than did those obtained with mercury, and this may, perhaps, be accounted for by the fact that in these the superficial area of the platinum electrodes was the same, whereas in the others the mercurial surfaces were not equal in amount.

	Difference of amount of deflection.
1. Cyanide of potassium (120 grs.)	·75
2. Cyanide of potassium (60 grs.)	·25
3. Formiate of sodium (120 grs.)............	·25
4. Carbonate of potassium..................	·25
5. Phosphate of ammonium	1·75
6. Hyposulphite of sodium..................	·25
7. Carbonate of sodium	very slight
8. Formiate of sodium (60 grs.)	2·0
9. Sulphite of sodium (60 grs.)..............	1·0
10. Sulphate of potassium	1·0
11. Sulphate of ammonium..................	very slight
12. Phosphate of sodium	·25
13. Borax	·75
14. Formic acid (96 minims)	1·0
15. Cyanide of potassium (6 grs.)	·75
16. Microcosmic salt (120 grs.)	·25
17. Nitrate of potassium	very slight

	Difference of amount of Deflection.
18. Nitrate of strontium	
19. Cyanide of potassium (3 grs.)	0
20. Sulphite of sodium (12 grs.)	·25
21. Nitrate of ammonium....................	·25
22. Microcosmic salt (60 grs.)	very slight
23. Sulphate of sodium	0
24. Selenate of sodium......................	·25
25. Nitrate of sodium	·25
26. Nitrate of barium	·25
27. Boracic acid............................	·25
	12·25
28. Ammonia alum (60 grs.)	
29. Selenic acid (12 minims)	·25
30. Selenic acid (24 minims)	·25
31. Ammonia alum (120 grs.)................	0
32. Sulphuric acid (1 in 80)	2·0
33. Sulphuric acid (1 in 40)	·5
34. Sulphuric acid (1 in 20)	1·0
	4·0

The total differences 12·25 and 4·0 in this series agree as nearly as could be expected with the maximum deflections 11·0 and 4·25 in the previous series.

Remarks.—The strength of the solution was found to have but little influence upon the *direction* of the current. Increase of strength of solution increased the amount of deflection in solutions of cyanide of potassium, sulphite of sodium, and selenic acid, and to a less extent in those of formiate of sodium, formic acid, microcosmic salt, and ammonia alum.

With the solutions of sulphuric acid, those of intermediate strength (1 in 40) gave a greater deflection at 180° F. than either a stronger or weaker one. No other instance occurred in which a solution gave a greater deflection at the temperature of 180° F. than a stronger solution of the same substance.

On comparing the results now found with platinum with those previously obtained with mercury, it will be seen that the two sets are very similar. The conclusions, therefore, which have already been drawn from the former apply largely to the latter, and need not be repeated. The chief conclusion, viz., that the currents previously obtained with mercury were really due to heat and not to minute amounts of chemical action, is largely confirmed by the present research. The chemical relations of mercury being considerably different from those of platinum in special cases, if the currents were due to chemical action, the two sets of results would have been widely divergent in their orders.

Agitation of the immersed metals is well known to influence the current. Shaking one of the metals reversed occasionally the direction of the current; palladium was the metal which most frequently manifested this effect. The agitation of that metal rendered it either more negative or less positive in a considerable degree, especially in solutions of potassic chloride and cyanide. In those of the bromide, antimony was the metal, the current from which was most affected by shaking. The effect of shaking was less in the iodide solutions than in those of the bromide.

Note.—Some years ago I made a note as follows: "If water is nearly saturated with a suitable salt at 15°C., and the upper part is then kept hotter and the lower part colder than that temperature for a long time, will the upper part become stronger and the lower weaker?" Recently, since this research was completed, the following statements have been published: (see "Nature," vol. xxii, p. 356, August 12, 1880, also vol. xxiii, p. 207, December 30, 1880) "Influence of Temperature on the Distribution of Salts in their Solutions," by M. Soret. "The concentration of the heated part diminishes, that of the cold increases. The difference grows with the original concentration, and nearly in proportion. In the series of the alkaline chlorides the difference is greater (for the same concentration) the higher the molecular weight of the salt. The phenomenon seems to have no relation to solubility of the salt." If these statements are correct (as is very likely) they affect to a slight extent all the results of the present research (and of other researches) on the thermo-electric behaviour of solutions, because they show that the chemical composition of the aqueous solution is slightly altered by the rise of temperature. If also the strength of the thermo-electric currents of liquids depends upon the degree of amplitude of vibration of the molecules of the salt, &c., in solution, it will probably vary, in the case of solutions of alkaline chlorides, with the degree of concentration of the solutions, and the molecular weights of the salts, like the phenomena in M. Soret's experiments.

VII. "Influence of Voltaic Currents on the Diffusion of Liquids." By G. Gore, LL.D., F.R.S. Received December 1, 1880.

(Abstract.)

The subject of this paper is an investigation of the effects produced by electric currents whilst passing through the surfaces of mutual contact of two electrolytes lying upon each other. (See "Proc. Roy. Soc.," vol. 30, p. 322.)

By an examination of about forty pairs of electrolytes of the most varied kinds, including solutions of various mineral acids, acid salts,

neutral and alkaline ones, &c., also of different degrees of dilution, the influence upon the effects of the kind and strength of the liquids, the strength of the current, and of various other circumstances, was determined. Two different apparatuses were employed in these experiments, viz., a single and a double meniscus one; the former was the most easily managed, and yielded the phenomena in their simplest form.

It was found that the phenomena are very complex, and consist of a mixture of physical and chemical effects. That they are chiefly due to electrolytic changes, to differences of specific gravity, to ordinary liquid diffusion, to electrolytic transfer and diffusion of liquids, and to heat of conduction-resistance. That also the mutual contact surfaces of the liquids act to some extent as electrodes and osmotic diaphragms. That by the electrolytic action, various phenomena, such as layers of liquid of different specific gravities, &c., lines, strata of colour, alteration of form of the surfaces of the meniscuses, liquid currents, &c., were produced. That by the heat evolved, expansion and streaming upwards and downwards of the liquids, evolution of gas and steam, and boiling of the liquids at their contact surfaces took place. Nearly all the phenomena and their conditions have been examined and discussed, but some of them, however, have not been fully explained.

The movements of the meniscus and apparent transfer of the bulk of the liquid were examined in a special apparatus in several ways, and a conclusion was drawn from these and the previous experiments, viz., that by the causes already named an apparent, and, also to a small extent, real bodily movement of the liquid did occur.

Some experiments were also made with two superimposed electrolytes in capillary tubes for the purpose of comparing the effects produced on passing an electric current through them, with those which occur in a capillary electroscope. The conclusion arrived at was, that the two classes of movement were considerably different, and that the movements obtained with the two electrolytes were more allied to those observed by Armstrong ("Phil. Mag.," xxiii, 1843, pp. 194–202), Quincke ("Pogg. Ann.," vol. cvii, 1859), Jürgensen and Wiedemann, on passing electric currents through non-conducting liquids in capillary tubes.

By means of numerous and varied experiments with apparatuses similar to that used by Porrett (Thomson's "Annals of Philosophy," vol. viii, 1816, pp. 74–76) in electric osmose experiments, the influence of electric diffusion of liquids upon the results was examined. From the results of these it was inferred that the apparent movement of the mass of the liquids was considerably different from that of electric osmose; in the osmose experiments it was found that the amount of liquid transferred was nearly always greater when the current passed from a weak to a strong solution of the same salt or acid than when it

passed in the reverse direction. This circumstance partly explains some of the phenomena observed in the double meniscus apparatus.

The influence of molecular structure of the liquid was discussed, and it was observed generally that the greater the degree of physical and chemical differences between the two liquids, the more conspicuous were all the effects.

The influence of viscosity, specific gravity, adhesion, and ordinary diffusion,' &c., of the liquids was also considered, and conclusions respecting them drawn, by the aid of the results of the experiments. It was concluded that they all affected the phenomena; that the phenomena of lines and apparent movement of the liquids were not wholly capillary; that although ordinary diffusion affected the results, its usual effects were modified by the current, which aided diffusion at the negative meniscus and neutralised it at the other.

Heat having been found to operate only as a disturbing element, the influence of rise of temperature was not specially investigated. The effects of heat resulting from conduction-resistance, and from chemical and electro-chemical action at the meniscuses were, however, observed; in some cases streams of liquid, evolution of gas and steam, and even the phenomena of boiling, were seen in the contracted portion in the midst of the mass of the liquids.

The electrical conditions were essentially important. The electric current produced both physical and chemical effects, consisting of electrolysis, dissociation of water from salt, and simple mechanical movement and diffusion of liquid by electric convection. As these phenomena appeared to be immediate effects of the current, either the current acted under conditions which were not uniform, and produced effects which required different degrees of electro-motive force to produce them, or the current consisted of several portions of electricity of different degrees of electro-motive force. The former supposition harmonised with the fact that the ingredients of chemical compounds were held together more strongly than those of mere mechanical mixtures. The physical portion of the effects were considered to result from electric convection due to electric charge and conduction-resistance at the surfaces of contact of the liquids, and in capillary tubes also at the surface of the glass. The unequal transfer of acids and bases, and the liberation of ions at the contact surfaces of the liquids, further influenced and complicated the effects. It was predicted as probable, that as the amount of electric osmose was greater from a weak solution of a salt or acid to a strong one than the reverse, conduction-resistances to currents of opposite direction passing through such an arrangement would be similarly unequal. Evidence was also adduced for the conclusion that every inequality of structure or composition of the liquid in the path of the current must act to some extent as an electrode. As no visible lines or movements of

liquid were produced without electrolysis, and electrolysis would reasonably account for several of the phenomena, it was concluded that electrolysis was not merely a concomitant circumstance, but also acted as a cause. The converse phenomena of the subject of this paper have been made the subject of a separate research, entitled "The Production of Electric Currents by Diffusion and Osmose of Liquids." No effect of magnetism upon the lines or movements of the liquids was sought for.

Chemical action was found to be only a coincident and not a fundamental part of the phenomena; it constantly took place by electrolysis and by contact of liberated ions, and occasionally by contact of the original liquids. The lines and strata of liquid at the meniscuses were produced with almost every possible chemical combination of electrolytes; and references are given to experiments which prove this; they were also the more difficult to produce the less the differences of chemical composition of the two liquids.

VIII. "Experiments on Electric Osmose." By G. GORE, LL.D., F.R.S. Received December 7, 1880.

The following experiments were made for the purpose of elucidating a question in a research on "The Influence of Voltaic Currents on the Diffusion of Liquids" (*ante*, p. 250), and are published separately, in order to facilitate reference to them by other investigators.

As, also, the discovery of an exceptional instance is often of considerable importance, an additional number and variety of solutions were purposely examined with that further object until such a one was found. I anticipated that the proportion of exceptional instances would be "about one or two per cent."

The experiments were similar to that made by Porrett ("Annals of Philosophy," vol. 8, p. 74). The vessel employed was about 7·0 centims. high and 3·0 centims. diameter, of thick glass, divided vertically into two equal parts, its edges being covered with vulcanised indiarubber, with a diaphragm of biscuit ware about 1·5 millim. thick placed between them; the whole being held together between two upright bars of wood by means of a clamp-screw, so that the diaphragm might be readily removed, and replaced by a clean one. The electrodes were of sheet platinum, about 18 millims. wide; and the electric current (except in the cases otherwise mentioned) was from 12 Grove's cells of one pint capacity, and in single series. The liquid was usually about 2 centims. high in the vessel, and at the same level on each side of the partition previous to passing the current in each experiment. The diaphragm was frequently changed.

The following table exhibits the compositions of the liquids tried,

and briefly the results. The electric current was in all cases sent from left to right.

No.	Substance.	Strength of Solution.	Direction of flow of liquid.
1	Yellow potassic chromate	Saturated solution	→
2	„ „ „	Very diluted solution	→
3	Distilled water..............	....	→
4	Potassic cyanide	1 gr. per oz..................	→
5	„ „	10 grs. „	→
6	Chromic acid (crystals)	1 gr. „	→
7	„ „ „	10 grs. „	→
8	Potassic nitrate	10 grs. „	→
9	Sodic nitrate................	10 grs. „	→
10	Strontic nitrate	5 grs. „	→
11	Baric nitrate................	5 grs. „	→
12	Ammonic nitrate..............	10 grs. „	→
13	Potassic sulphate............	10 grs. „	→
14	Ammonic sulphate	10 grs. „	→
15	Ammonic alum.................	10 grs. „	→
16	„ „	5 grs. „	→
17	Potassic sulphate............	Saturated solution	→
18	Oxalic acid	63 grms. per litre	→
19	Nitric acid	1 volume of acid, and 5 water	→
20	Acid sulphate of sodium	Saturated solution	→
21	Glacial phosphoric acid	400 grs. per oz...	→
22	Potassic carbonate	5 grs. „	→
23	Sodic carbonate	5 grs. „	→
24	Borax	5 grs. „	→
25	Microcosmic salt.............	5 grs. „	→
26	„ „	10 grs. „	→
27	Sodic phosphate	10 grs. „	→
28	Ammonic phosphate	10 grs. „	→
29	Potassic sulphite............	5 grs. „	→
30	Sodic sulphite	5 grs. „	→
31	„ „	1 gr. „	→
32	Sodic hyposulphite	5 grs. „	→
33	Potassic chloride............	2 ozs. salt in 12 ozs. water....	→
34	Potassic bromide.............	200 grs. per oz..............	→
35	Sodic formiate...............	5 grs. „	→
36	„ „	10 grs. „	→
37	Potassic ferrocyanide	1¼ oz. salt in 12 ozs. water....	→
38	Sodic selenate	2½ grs. per oz.	→
39	Nitrate of lithium	Strong solution	→
40	Calcic nitrate	„ „	→
41	Selenic acid	2 minims. per oz.............	→
42	Formic acid	8 „ „	→
43	Iodic acid..................	Moderately strong solution....	→
44	Perchloric acid..............	Strong solution..............	→
45	Chloric acid	„ „	→
46	Hydrochloric acid	1 volume acid, 5 water	→
47	Potassic hydrate	¼ oz. in 14 ozs. water	→
48	„ „	5 grs. in 100 c.c. water	→
49	Sodic silicate...............	Rather weak solution	→
50	Potassic chlorate	„ „ „	→
51	Ammonic chloride	1 oz. in 8 ozs. water	→
52	Ammonic molybdate with aqueous ammonia	....	→

No.	Substance.	Strength of Solution.	Direction of flow of liquid.
53	Sulphocyanide of ammonium..	Strong solution	→?*
54	Tungstate of potassium	Saturated solution	→
55	Phosphorous acid	Strong solution..............	→
56	Arseniate of sodium	....	→
57	Potassic fluoride (pure)	....	→
58	Liquor arsenicalis	British Pharmacopœia strength	→
59	Hypophosphorus acid	Saturated solution	→
60	Sulphocyanide of potassium ..	Strong solution	→?*
61	Ferridcyanide of potassium....	Moderately strong solution ...	→
62	Sodic hydrate...............	Strong aqueous solution	→
63	Potassic cyanide............	Saturated solution...........	No perceptible flow.
64	Sodic carbonate.............	" "	→
65	Potassic carbonate	" "	→
†66	Sodic hydrate	200 grs. in 1 oz. absolute alcohol	→
†67	Hypophosphite of sodium	Strong alcoholic solution......	→
†68	Baric bromide	Nearly saturated alcoholic solution	←

In all the foregoing experiments (sixty-eight in number), in which an electric current passed from one portion of liquid through a clay diaphragm to another portion, osmose occurred, except in No. 63.

In these experiments, fifty-five substances, different in kind, were employed, including about twenty-seven neutral salts of the most varied composition, about ten alkaline ones, twelve acids, two alkalies, and one acid salt; twelve of the substances of different degrees of dilution were also examined.

As with solutions of potassic cyanide, yellow potassic chromate, acid chromate of potassium, or sodic carbonate (liquids which show an apparent opposite movement in the research referred to), the osmose was in the same direction as the electric current, and showed no sign of reversal of movement, I conclude that the apparent movement of the lower liquid in that research is probably of a different character from "electric osmose."

From these experiments, it also appears that the direction of electric osmose is in nearly all cases the same as that of the current; that rapid electric osmose is not confined to dilute solutions; and that in some cases too concentrated a solution prevented the effect (compare Nos. 17–20, 54, also 4, 5, and 63).

From the circumstance that the direction of osmose was the same in

* In consequence of the large quantity of orange-red solid matter liberated, the direction of the osmose could not be determined.

† In these three experiments a current from a single series of 25 Grove's cells was employed.

the most diverse solutions, viz., in acids, alkalies, neutral salts, aqueous and alcoholic liquids, concentrated and dilute solutions, &c., we might be apt to infer that the chemical composition of the electrolyte had no influence upon it; but as a single exceptional instance will overturn the widest generalisation, so the exceptional behaviour of a solution of bromide of barium in absolute alcohol invalidates the conclusion that the direction of flow of liquids in electric osmose, is independent of the chemical composition and molecular structure of the liquid. The danger of drawing conclusions from too limited a number of instances is well illustrated in this case, especially when we further remember that it is the exceptional instances which usually disclose the widest truths.

As the exceptions formed a very small proportion of the whole number of examples, it would appear that the direction of the flow depended very much more frequently upon the direction of the electric current than upon the internal architecture of the liquid.

In order to be able to compare the direction of motion of the mass of the liquids, produced by passing an electric current from a heavier to a lighter liquid lying upon it without a separating diaphragm, in the research already referred to, with that produced when the liquids were separated by a vertical diaphragm, additional experiments were made. The osmose cells employed in these experiments were about 4·7 centims. high, 2·5 centims. long, and about 2·0 centims. wide at right angles to the diaphragm; and the diaphragms were cemented in with sealing-wax.

Experiment 1.—Current from 26 Grove's cells in single series, passed from a saturated solution of sodic sulphate to a one-fourth saturated one of potassic chloride. Osmose occurred in the direction of the current.

Experiment 2.—Current passed from a mixture of 1 volume of sulphuric acid and 7 of water to one composed of 5 volumes of a saturated solution of oxalic acid, and 3 of water. Osmose produced in the usual direction.

Experiment 3.—Current passed from a strong solution of potassic chloride to a mixture of 1 volume of a saturated one of ammonic sulphate and 3 volumes of water. Rapid osmose took place in the ordinary direction.

Experiment 4.—Current from 5 Grove's cells in single series, passed from a solution composed of 1 volume of a concentrated solution of sodic hydrate and 3 volumes of water, to a saturated one of sodic carbonate. Osmose occurred in the usual direction.

Experiment 5.—The same current, passed from a strong solution of ammonic nitrate to one of 1 volume of a saturated solution of sodic carbonate and 3 volumes of water. Feeble osmose in the ordinary direction took place.

From the results of these experiments, and of those referred to, it appears that the presence of the diaphragm considerably affects the directions of the movements.

By means of numerous experiments subsequently made, it was also found that electric osmose usually proceeds more rapidly from a weak to a strong solution of a given substance than in the reverse direction.

The Society adjourned over the Christmas Recess to Thursday, January 6th, 1881.

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[Plates 3 and 4.]

In the present communication it is intended to advert to the subjects—

(1.) Minimum visibility.

(2.) The effects of excessive angular aperture in obliterating minute molecular structure.

(3.) Minute measurement and a micrometer gauge.

(1.) *Minimum Visibility.*

Spider-lines miniatured down to the fourteenth part of the hundred-thousandth of an inch have been made distinctly visible to ordinary good eyesight under proper microscopical manipulation. The question then arose whether an actual thing so small as the millionth of an inch could be descried in the microscopic field of view.

According to recent researches, it would appear from formulæ derived from the undulatory theory of light, that brilliant disks are developed from points of light which vary inversely in diameter with the increase

of angular aperture of the objective employed, and directly as the wave-length of the kind of light employed.*

Professor Helmholtz and Professor Abbè have independently arrived at this beautiful law. And Professor Helmholtz cautiously states, however, there may be some conditions which may modify this law.

It follows from this principle that thin brilliant lines of light can be best separated by glasses of the highest angular aperture; and the separating power can be measured by the sine of the semi-aperture. Now for wave-length $\frac{1}{50000}$† we may thus tabulate them (when $\frac{1}{2}\lambda = 100{,}000$):—

The Values of ϵ for Different Apertures.

Aperture. °	(ϵ,) the limit of proximity of bright lines or disks.
180	100,000th of an inch.
160	98,480th ,,
140	93,970th ,,
100	76,600th ,,
60	50,000th ,,
40	34,200th ,,
20	17,300th ,,
15	13,000th ,,
10	8,700th ,,
5	4,360th ,,
1	870th ,,
½	436th ,,

Such values as these have accordingly been generally accepted as limits to the resolvability of close lines with objectives of given apertures. Further, it is said that lines drawn at the rate of a hundred thousand to the inch represent the limit of microscopic visibility.

In a paper on this subject by the author it has been shown that a bright space enclosed between two spider-lines, miniatured so as to form a bright interval $\frac{1}{350000}$, was distinctly visible, whilst the webs actually were about the $\frac{1}{5000}$ and $\frac{1}{7000}$ of an inch in thickness.‡

Under these circumstances, it was interesting to determine whether

* $\epsilon = \dfrac{\lambda}{2 . \sin \alpha}$.

λ = wave-length; α = semi-angular aperture; ϵ the distance between the centres of the disks in contact.

† Sir John Herschel estimates the wave-lengths as follows, in parts of an inch—

λ......	Green	47,560th.
λ......	Intermediate	49,320th.
λ......	Blue	51,110th.

In round numbers, for bright blue illumination, $\lambda = \frac{1}{50000}$.

‡ "Proc. Camb. Phil. Soc.," vol. iii, p. 217.

real objects could be detected by the microscope of the surprising degree of attenuation represented by the millionth.

For this purpose a long search was instituted for many months, both for the finest attainable defining powers and for suitable objects. Minute mercurial particles have appeared to furnish very interesting evidence. If these be very finely smashed with a small piece of watch-spring, the experiments will be promoted by inserting a minute drop of petroleum beneath the "cover," which should be very thin and clean (fig. 9).

A reflex illuminator is then used to converge rays downwards through the objective upon the preparation.

The most extraordinary minute disks (in a darkened room) may be then observed (of a circular form chiefly). Some of them are irregular, and *upon* some of them will be detected clusters of the most wonderful degree of smallness, consisting of minute black points, visible with a power of 1,000 diameters. And what is so surprising is the entire absence of any diffraction effects at the edges of these black points.

For convenience of ready reference, I have found micrometer spider-lines of service for comparative estimation. There are three thicknesses carefully measured:—

No. 1	1- 5,000th of an inch.
No. 2	1- 8,000th ,,
No. 3	1-10,000th ,,

Comparing these black particles floating on the surface of the mercury with the apparent diameter of the thinnest spider-line, some of them appeared decidedly smaller. This observation has been confirmed by several persons to whom they were exhibited.

Under 1,000 diameters the particle was magnified just one hundred times in the micrometric focus. It then appeared less than the spider-line $\frac{1}{10000}$ of an inch thick. Its real diameter was therefore less than the $\frac{1}{100}$ of this magnitude, or less than *the millionth of an inch*, a microscopic unit which I propose here to represent by $\mathfrak{m}$. Under a power of 2,000 diameters the finest spider-line would represent the quantity $\frac{1}{2}$ $\mathfrak{m}$.

The accompanying drawing fairly represents the appearance of the spider-lines seen on a dark field when mercurial particles, as already described, are examined with the micrometer under a power of 1,000 (Plate 3, fig. 9). (The webs appear too coarse in the plate.)

A positive eye-piece utterly spoils the finest defining powers, and a Huyghenian is generally substituted.*

The very beautiful phenomena observed by means of brilliant illumination of these minute mercurial disks, flattened by pressure or

* Definition may be further improved by substituting a crossed lens for the field, and an achromatic magnifier for the eye-lens.

moving freely in the fluid, I propose to describe in another portion of the present research. Suffice it to say, that they fairly and surprisingly imitate the starry lustre of a midnight sky—the Milky Way and the various phenomena of the closest double stars.* They display too under the finest attainable amplification the most accurate diffraction rings I have ever witnessed, and as such give admirable indications of quality and correction in the lenses employed. In this case floating mercurial molecules,† uncompressed, reflect astonishingly minute focal images.

From these observations the conclusion may safely be drawn that real objects of unsuspected minuteness may be microscopically displayed, as well as minute miniature images, such as I have elsewhere described.‡

There is only one objection to this result. It may, perhaps, be said that bright diffraction diminishes the apparent diameter of these black points. But they may be observed almost touching each other. The great difference in diffraction in the case of black particles seen on a flat illuminated disk of mercury, and black particles seen by transmitted light, so soon as the objects are illuminated from beneath, is most striking. Besides this, continued observations of another kind confirm the conclusion arrived at. The question of the *minimum visible* regarded as visual angle is of equal importance in telescopic as in microscopic researches, and is well illustrated by the appearance of minute stars in large telescopes of great excellence. There the actual angular aperture is infinitesimally small, as the rays enter the instrument in a state of parallelism. As, however, an object may be supposed to move up towards the instrument, the angular aperture gradually assumes a tangible quantity. For instance, a fly's foot viewed with Lord Rosse's gigantic reflector of 6 feet diameter, formed an angular aperture of 5° 44″, the object being 60 feet distant. At a certain point, as the focus is shortened, a telescope becomes a long focus microscope. The instruments are, indeed, convertible. A very long focussed microscope is in reality a shortened telescope. At this point the laws of vision apply identically, and the angular aperture is the same for each. The question, then, of vision in the microscope, when only small aperture is engaged as in the telescope, may aptly be illustrated by minute astronomical observations. The eye, too, engages a near object at a small angular aperture: the pupil is then the base, and the object the apex of an isosceles triangle measuring the angular aperture, as much perhaps as a couple of degrees in short sight, but infinitesimally small as in the telescope when directed to a star. The

* Great pains were taken to secure brilliant illumination in a dark room.

† These very minute mercurial globules move freely, as though floating.

‡ "Phil. Trans.," 1870; "Month. Jour. Roy. Mic. Soc.," "Testing Objectives by Miniatures of Mercurial Globules," vol. i, 1875; "Limits of Vision," vol. i and vol. ii, 1876.

limit of visibility measured by visual angle is reached, equally by the eye-lens of a microscope or telescope, or by a simple magnifying glass, when the object subtends too small an angle to be sensible to the retina.* Both instruments require identical eye-pieces. (Two seconds of arc is the limit in general of human vision.)

Comparing, then, the minimum visible in both instruments, and beginning with the telescope, it may be urged that the spurious disks of the minutest stars visible, such as the 17th or 20th magnitude, must subtend an exceedingly small visual angle.

It is then fair to inquire under what visual angle such disks can be glimpsed in an eye-piece, used indifferently in either microscope† or telescope. Great loss of light in defining these minute points is prevented by very accurate curvature in a reflecting mirror.‡ Accordingly the Uranian satellites have been seen in an 18-inch silver glass reflector. The extreme minuteness of these bodies is illustrated as follows:—

A double star between β_1 and β_2 Capricorni may be discovered whose components are of the 16th and 17th magnitude. Yet Sir J. Herschel declares, that in comparison with the Uranian satellites, these two minute stars are splendid objects. The visual angle at which these satellites are seen in Calver's 18-inch mirror must be exceedingly small indeed§ (see mercurial double stars, fig. 9).

Sir John Herschel records an extremely fine observation of the double star Σ_2, composed of two minute equal stars nearly as small as the 9th magnitude; seen at the Cape with a 20-feet reflector armed with a 24-inch mirror:—

"Charmingly divided with 320; the disks, like two grains of

* The spurious disks of stars of excessive minuteness are very much smaller than those of the larger kind. But the usual mathematical expression for this size is independent of star magnitude, and is in this respect incomplete. Such disks are therefore matters of observation, and their size in some degree conjectural.

† The mercurial illuminated star disks are here alluded to.

‡ The same economy of light is secured in microscopic object-glasses of great perfection, which disperse the otherwise inevitable white fog of spherical aberration. The visibility of these Uranian satellites in Calver's 18-inch silver glass mirror is doubtless due to a similar cause.

§ Compare this with the satellites of Saturn, 280,000,000 distant:—*Dione*: diameter, 500 miles; visual angle, 0″·85; magnitude, 12. *Tethys* is reckoned magnitude 13; *Enceladus*, 15; *Mimas* and *Hyperion*, 17. If the law could be applied, that the light being the same, visibility varies as the square of the diameter, and the next magnitude is half the magnitude preceding, the visual angles subtended by the Saturnian satellites might be guessed at with a power of 500:—

	Rhea.	*Dione.*	*Tethys.*	*Enceladus.*	*Mimas and Hyperion.*
Magnitude......	10th	12th	13th	15th	17th
Visual angle	160″	67″	47″	23″	16″

And this very nearly agrees with the limits assigned.

mustard seed, separated by one-third the diameter of either. *Vicinæ* according to Struve, but at least *per vicinæ:* dist. between centres 2-3rds of a sec."*

Recurring again to the law of brilliant disks in the microscope diminishing in diameter with the increase of objective angular aperture, I beg to call attention to the observation that in examining miniatures reduced 1,000 times, the details of these miniatures appeared entirely free from diffraction effects so long as the light was subdued. But the instant the sun shone every reflecting point or polished surface became entirely disguised, concealed, or obliterated in its details by the strong diffractions.† It is on this account that mild daylight observations from light received from a white cloud have been found so serviceable in minute research as already described.

Unfortunately, the attainment of the most delicate microscopic

* From these data, if $6x$ be diameter of each, $2x$ is the dividing space or "black division," so that

$$3x + 2x + 3x = \tfrac{2}{3}''.$$

Whence

$$6x \text{ or diameter of disk } = \tfrac{1}{2}'',$$

$$2x \text{ or division } = \tfrac{1}{6}''.$$

Hence the angle subtended by either disk with power 320,

$$\theta = 320 \times \tfrac{1}{2}'' = 160''.$$

The angle subtended by dividing black line,

$$\theta = 320 \times \tfrac{1}{6}'' = 53''.$$

The last result corresponds to a black line 1-400,000th of an inch magnified 1,000 times in the field of the *microscope.*

A short table is here inserted for reference. If the millionth of an inch be taken as the microscopic unit of reference (m), and a particle or line of this size be magnified 1,000 times, it will subtend to the eye of the observer, as an image 10 inches distant, an angle *ten times greater than the human limit,* viz.:—

20″·6265.

Diameter of object.	Visual angle.	Power.
1-10,000,000th.............	″2	1,000
1- 1,000,000th.............	20½	
1- 500,000th............	41	
1- 400,000th............	51½	
1- 300,000th............	69	
1- 200,000th............	103	
1- 100,000th............	206	
1- 50,000th............	412½	
1-1,000,000th..............	41	2,000
1-1,000,000th..............	60	2,500

† See "Circular Solar Spectra," by the writer, "Proc. Roy. Soc.," vol. 21, p. 426

research is beset with many difficulties and variable factors, some of which may easily be overlooked or even unsuspected.

As nearly all organic tissues teem more or less with minute molecules, their variable behaviour and optical appearances have received close attention for many years, and I have concluded that—

A.—A refracting molecule changes its appearance and phenomena according to the nature of the fluid by which it is surrounded, also with the fluid in which the objective may be immersed.*

B.—It changes also, in an extraordinary manner, according to the angular aperture of the objective employed, according to the refractions of the media, and the direction of illumination and the parallelism or degree of convergence or divergence of the illuminating pencil transmitted.

C.—Other interesting factors are the residuary aberrations of the compounding lenses of observation and the greater or less intensity of diffraction phenomena introduced by the mode of illumination.

In a short paper, it will be convenient partly to deal with these questions as they arise in the experiments rather than *seriatim*.

(2.) *Excessive Angular Aperture considered.*

The principal and chief, I may say the most valuable, feature in the appearance of a refracting molecule is the extraordinary variability of the blackness and thickness of the *marginal annulus*.

This thickness and consequent visibility is dependent on the value of the refraction into the given media, and the angular aperture of microscopic observation: partially also upon the situation of the focal point of the lenticular illumination.

Example 1.—A glass spherule (0″·1 diameter) is examined with a pocket lens. An intensely black broad ring is seen against the light. The same black ring is visible in bubbles frequenting plate glass in windows. The angular aperture of the pupil of the eye is about a degree for an image seen at 10 inches distance. Melted glass filaments are also instructive (see figures α, β, γ, δ, ϵ, ζ, η, θ).

* It is not many years since lines on diatoms were all that were searched for. But as these are formed by an aggregation of siliceous spherules, they present extraordinary opportunities for investigating optical characters which must necessarily belong to them—of a degree of minuteness of a highly satisfactory order—such as black annuli, focal points varying with chromatic refraction, shadows varying in contour according to the extinction of transmitted rays by obliquity of illumination: appearances changing with the refractive index of the media in which they are immersed. Only the very finest glasses extant can display the black margin of a diatomic spherule the $\frac{1}{10000}$ of an inch in diameter; and very few, if any, can be obtained capable of displaying focal points and the natural coloured foci at different chromatic foci.

Example 2.—A microscope with ten degrees aperture considerably diminishes the relative thickness of the black annulus.

Example 3.—With forty degrees it is much attenuated and it vanishes altogether when the angular aperture reaches a certain relation to the lenticular refraction.

The vanishing limits vary with the nature of the refracting spherule, and also with the angular observing aperture and the direction of the axis of illumination.*

If it be formed of plate glass whose index of refraction into air is 1·500, the vanishing angular aperture is 83° 36′. But as the index is higher, this angle increases with heavy flint glass ($m=1·988$) to 164° 6′. In both these cases the black defining annulus of the spherule is in these limits attenuated almost to evanescence.

Glasses, therefore, of small angular aperture develope the broadest black outlines in a minute refracting molecule.

The change of appearance of translucent bodies composed of masses of refracting molecules is very finely shown in observing a variety of scales forming the dust of moths and butterflies.

Example 4.—*Featherlets of the death's head moth. Low angular aperture*, 10°. The whole animal bristles with black feathers, armed with three or four long black spines: all of a dark but rich umber colour, tipped intensely black; each spine shows an exceedingly thin line of light running centrally up between two broad intensely black margins. (Exactly what is seen when examining a thread of spun glass with low aperture.) (Fig. 1, Plate 3.)

Increasing Aperture.—Colours pale. Light flashes through. The spherules begin to appear edged with black annuli, which gradually attenuate with higher angled glasses. (Direct light.) (Fig. 2.) (Fig. ϵ, ζ.)

It is remarkable how the colour changes as the aperture increases, through paler shades, until a general sparkling radiance appears to steal through the mass of molecules formerly darkling with the universal presence of black annuli due to low aperture. The black edgings also of terminating membranes extending from spikelet to spikelet, and those of refracting tubules become indistinct.

The obliteration of marginal shadows is well shown by first using low and then large aperture on another very beautiful object, viz.:—

Example 5.—*Plumelets of the Hipparchus Janira. Aperture* 20°. Power 200. The filaments of the plume and their clubbed ends are all intensely black (Plate 4, fig. 13*a*).

* "The use of a new Aberrameter, for testing Aberration and the Effect of Aperture," "Quart. Jour. Mic. Science," January, 1871. The angles subtended by the black shadow are there calculated and tabulated.

Aperture 44°.—All still intensely sharp and black with the fine definition of a Wray "half-inch."

Aperture 55°.—Wray glass. Margins thinner; the filaments begin to be translucent and at the spherically clubbed ends a focal point of light appears.

Aperture 94°.—A remarkably fine 1-6th by Bénechè of Berlin. Power 600. Increase of translucency at every part; black margins attenuated. Club margin much thinner (fig. 13*b*).

Aperture 140°.—Fine 1-8th. Power 800. Black margins almost attenuated to invisibility; clubs translucent altogether; no annulus.

It is interesting to state that the thickness of the filaments of this plume vary between $\frac{1}{80000}$ and $\frac{1}{90000}$, yet these are beautifully distinct (aperture 12° and 50 diameters 2-inch objective), and yet under this amplitude a single filament subtends* less than 20 *seconds of arc.*

The exceedingly black and sharp appearance of these filaments doubtless accounts for their actual visibility under this very small visual angle. Now this exactly represents a line 1-1,600,000th thick (considerably less than a millionth of an inch) seen under a power of 1,000 diameters.†

If then the minute fibrillæ of the plume can be clearly distinguished as closely packed black lines, at a visual angle of twenty seconds, with low aperture of twelve degrees, this result is fatally opposed to the popular idea that very close lines, or very minute lines or bodies, can only be distinguished with large angular aperture. These lines were most sharply seen, though less than 1-80,000th thick (fig. 13*a*, Plate 4).

But besides the black sharply defined ring or annulus always developed in a refracting molecule by using low aperture, oblique illumination produces a thorough change in this black ring: it is transformed into various black crescents; and a row of such molecules approximately appears as a continuous black line sometimes notched. A tubule also produces a variety of marginal shadows according to the angular aperture of observation and according to the arrangement of internal molecules, and their combined shadows produce a variety of effects of an important character (fig. 6).

It follows from these considerations that researches upon inexhausted structures (those inadequately resolved for instance), must be conducted with especial reference to the development and detection of shadow annuli or bands, or notched black lines, and with special

* $\theta = \frac{\text{arc} \times \text{power}}{\text{rad.}} = \frac{\frac{1}{80000}}{10 \text{ inches}} \times 50 = \frac{1}{16000} \therefore \theta = 13''.$

† $\theta = \frac{\text{arc}}{\text{rad.}} \times \text{power} = \frac{1 \div 1600000}{10} \times 1000 = \frac{1}{16000}.$

regard to the angular aperture of the most effective kind, whether modified by the limitation of illuminating rays or by reduction of aperture; or by the refractive media concerned, viz., immersion fluids and "mountings." (Fig. 10).

Very striking examples of the disappearance of distinctive shadows, and consequent obliteration of structural molecules, are afforded by the coarser Podura scales; principally due to the use of excessive angular aperture (figs. 3, 5).

It is now eighteen years since a single observation suggested the present research, which has been followed up almost continuously towards the attainment of transcendent high power definition.

Dark molecules suddenly started into view under accidental manipulation, but were most difficult of reproduction, and in finer objects of the same kind were often utterly unattainable.

The questions naturally arose—are certain optical zones in the objectives more effective; the spherical aberrations existing there as a minimum; or are there other occult causes of occasional yet splendid definition under high powers? This did not appear at all to be a question of *the minimum visible* because the objects exceeded the 1-50,000th of an inch in diameter.

These points may be illustrated by the records of some observations made by the writer.

Although the molecules of the scale from the insect *Podura domestica* are large, the 1-45,000th, a stringlet of these baffles the powers of the finest glasses now extant, as ordinarily employed, whilst those of the finer test scales (the *Podura curvicollis*) are hopelessly attempted by every observer who trusts to excessive angular aperture (*Pod. domestica*, fig. 3, Plate 3).*

Example 6.—*Data for Resolution.* 2-3rd objective as condenser: a U-shaped stop aperture placed on its front lens.† Powell and Lealand's 1-50th objective. Direct light of petroleum lamp.

Result.—A grand display of long rows of whitish molecules in continuous contact (power 2,500), fig. 4, and between these rows are seen closely packed rouleaux of a dark lead-blue colour in a parallel higher plane. The molecules appear like a pearl necklace, in stringlets of twenties and thirties, which can be easily counted, as each molecule appears to be about three degrees of visual angle in diameter.‡

Remove the U-shaped stop, and the whole beautiful resolution disappears (fig. 5, left half). If, now, instead of using this difficult

* See Plates 73, 74. "Monograph of the Collembola and Thysanura," by Sir John Lubbock, Bart. (Ray Society).

† This method causes a slight obliquity of illumination, besides considerably reducing the angular aperture of the objective. Aperture of condenser 30°.

‡ $\theta = \frac{1}{45000} \div 10 \times 2500 = \frac{5}{90} = 3°$ nearly.

glass, a very fine 1-8th is used, at the same time that very oblique light illuminates the object, by tilting the axis of the condenser, black crescentic annuli are clearly developed (fig. 6). There is no doubt the view manipulated is entirely due to shadow thus developed and the preservation of marginal darkness. In this way the object may be shown with a very fine half-inch objective. The molecules are indeed huge compared with other visibilities: perhaps the explanation here given may enable other observers to confirm their existence in this comparatively easy object.

The magnificent *Oil Immersion Lens*, by Zeiss, of Jena, utterly fails with its full aperture to show the appearances just described. It displays grandly indeed the long corrugations, tubules, or ribs crossing each other irregularly, but entirely misses their beautiful contents: fully charged though they be with spherules (see fig. 7, Plate 3).

The total disappearance of this interesting structure, on the field of such a glass, is both surprising and instructive to the beholder.

Experiment 7.—*Data of Resolution.* Inferior 1-8th P. and L. 140° Objective, 1862. Aplanatic condenser 44° of aperture (Wray half-inch). February 5th, 1879. Power 800: cloudy daylight. Direct light.

The ribs are interrupted and alternately mottled blue and rose colour, slightly pink-coloured molecules without shadow annuli just discoverable peeping between the interrupted ribs. (The lowest focal plane must here be diligently searched.)

Example 8.—*Data.* Objective by Powell and Lealand of exceedingly fine quality. (Screw collar has 2½ turns and was opened to 25° from zero.) Cloudy daylight; Same illumination.

Result.—Eidolic black dots very much smaller than spherules discernible between very decisive appearance of spines or exclamation markings (! ! !). At a very high plane of focal vision, spines alone, the familiar optical test (fig. 8). *This singular result requires investigation.* Eidolic black dots are generally discoverable on a focal plane above the centre of refracting molecules.

Example 9.—Newest form of P. and L. 1-10th immersion, a glass of remarkable precision.

Lowest attainable focal plane, with "collar" fully open, so that "nose" is in contact with the "cover." Daylight as before. Aperture of direct illuminating cone 44°, formed by Wray half-inch.

Result.—The spines are broken into *double rows of minute black dots.* Shadowy white beads are glimpsed (with most scrupulous attention to focal and collar adjustments) between the spines. But when the condenser is tilted, strong dark crescentic annuli suddenly appear on the shadowy whitish molecules.

Example 10.—*Data.* 3 P.M. Sun dimly visible: Powell and Lealand's diaphragm No. 2, diameter 0·2 to reduce angular aperture of

Zeiss oil lens used as condenser. Objective Wray 1-10th immersion of extraordinary precision of definition; same object as before.

Result.—Whenever the sunshine was dimmed with clouds both spaces between the spines and the spines themselves are thoroughly resolved into molecules at the same instant. (A very difficult operation.) A slotted U-stop replacing the small diaphragm aperture now displayed both black and yellow molecules, the spines having totally disappeared.

As it now seemed only fair to Herr Zeiss' splendid "*oil immersion*" $\frac{1}{10}$, the aperture of the achromatic condenser was reduced to 44°, its axis being coincident with the optical. In place of using cedar oil the glass was immersed in a mixture of Rangoon and olive oil. Long straight chains of red beads appear shaded black at their margins, and yellow ones dotted black and in a lower focal plane (fig. 10).

Shaded ribs filled with spherules finely seen with 1-16th imm. Gundlach. The aperture of this glass was further reduced by using only a five-inch tube, and separating the front lenses as much as possible. This also corrected the spherical aberration (introduced by the tube being shortened) as well as reduced the angular aperture.

In these very difficult resolutions, skill, even in full practice, frequently fails.

Fortune may, however, surpass it, and I may record here a remarkable instance of it:—

Herr Bénechè, of Berlin, has made for the writer a very excellent glass, which, however, performs best with direct central light (1-16th immersion objective). It will be now seen how important a factor in delicate observations is the thickness of the "cover" glass.*

Example 11.—*Data.* Bénechè. Sleeting sky—same object. The nose touches the cover; slight action of the screw collar compresses the spherules. The Podura oil, natural to these scales, is seen slowly to permeate within the now flattened sac forming the scale. Rows of molecules vanish one after another. Upon releasing gently the slight pressure, they reappear at will one at a time. A molecule remaining still in contact with the cover appears dark. They lie in rows, by twos and threes, and here and there form V's by anastomosis. Placing the correction collar at the most favourable point for finest definition, intensely dark, almost jet-black, spherules appear like shot to occupy each rib. But the scale appeared utterly spoiled by pressure. The next day, however, it had entirely recovered its former appearance by the force of capillary attraction. Again, the native oil was squeezed gently along its many channels at a venture, by means of the fine focal adjustment.

* Bénechè particularly insists, very properly, upon this: as, after all, the collar adjustment is but a coarse compensation for the peculiar aberrations introduced by a thin plate of glass of varying quality and thickness.

Result.—Many V-shaped rows are visible: double molecules here and there cross between the ribs. Rapid but delicate changes of pressure cause minute oil globules to flash up and down with focal points.

An interesting demonstration of the molecules completely filling up the scale, may be given by rotating the object so as to receive oblique pencils successively. The black crescentic shadows of the molecules may then be seen in different positions relative to the ribs of the scale (fig. 6). Fig. 10.

I must confess that in my earlier attempts to account for the extreme and often insurmountable difficulty attending the demonstration of minute crowded molecules in many translucent organic forms, it was erroneously concluded that residuary aberration was the only cause of their obstinate obscuration. I am now able to amend this conclusion by adding another obstacle, *excessive angular aperture, which I found attenuated margin.*

Indeed, upon patiently reducing the residuary errors to a minimum, by means of supplementary lenses, as described in the Transactions of the Royal Society,* it was further discovered that the mode there described very considerably reduced also angular aperture. This result necessarily followed the very considerable shortening of the posterior foci of the objectives employed: at the same time, notwithstanding the full aperture of the lenses is engaged, the residuary aberration of the whole is reduced also, in consequence of angular aperture being at the same time reduced: this large aperture being indeed the great source of spherical error.

The angular aperture may be reduced in the same objective by introducing stops between the back sets of lenses. It is better, however, to have a series of objectives of excellent quality of graduated apertures. Aperture can always be reduced by shortening the eye-tube, or by using low power objectives and deeper eye-pieces to get the same power. It may also be reduced in a manner by using an illuminating cone of light, whose aperture can be diminished at will.

There is, it may be said, something unnatural in the mode of vision intrinsic to very high angled glasses. It is undoubtedly true that such a glass presents an *all-round vision.* It really conveys visual rays from a given brilliant particle, at every inclination in azimuth and altitude; and this too at one and the same instant.

To illustrate this position, a minute die may be imagined the hundred thousandth broad. The highest angled objective really enables the

* "Phil. Trans.," vol. 160, 1870. "On a Searcher for Aplanatic Images," by the Author.

observer to collect rays emanating from *four sides*, and the top at the same instant. The human eye could at most view *three sides* at once. Doubtless, the effect of this angular vision all round the corners, causes particles to look spherical when sufficiently minute, even if cubical.

To these considerations we may add the complete attenuation of annuli (which are black in refracting molecules with low angular aperture), and the comparative invisibility of masses of molecules.

It may be remarked here, that the resolutions described have chiefly been accomplished by placing the objects in a condition to develope black annuli or crescentic shadows, and chiefly by reducing the aperture. It is also remarkable that the popular markings of the Podura scale lie in a higher plane than the molecules, and having been sought hitherto in a wrong focal plane, were naturally missed.

In cases, however, where the composite spherules are much more crowded and much more minute, mere reduction of aperture is inadequate to discover their existence. Their excessive delicacy of form and translucency of substance, the difficulties introduced by unmodified diffractions, and by searching the wrong focal planes, mismanagement of shadow-phenomena and uncorrected residuary aberration combine together to present such formidable obstacles to success, that few can congratulate themselves on the complete resolution of this celebrated test object.

[It having been pointed out that the apparent beadings on gnat scales have been degraded by the application of the term spurious, it may be said in reply that no proper explanation of these appearances has been given. On the other hand, spherules much finer may be distinguished on several delicate scales.

Mr. De La Rue pointed out many years since the beaded character of insect striated scales;* Mr. Stephenson has minutely described those of the gnat scale; and Mr. Slack other beaded scales, in "Student," vol. v.

In order to place the matter beyond reasonable doubt, and to clear up the structure, diligent search has been made among a considerable variety of these objects, and in the 2nd Plate drawings are given from

* "Trans. Mic. Society," vol. iii, December 20th, 1848. In this paper Mr. De La Rue was able to show that with a 1-12th Ross, the ribs of the butterfly scales of *Amathusia Horsfieldii*, were from the 1-6,000th to 1-10,000th apart centre to centre, and that cross striæ, under a power of 850, showed protuberances on beaded lines, and the latter focussed at their summits appeared as brown dots. The scale viewed from the under side exhibited the lower membrane as slightly undulating, probably from being dry, p. 86. He proceeds: "Some of my friends thought the constricted appearance of the cross striæ is due to overlaying pigment cell: this, in my opinion, is not correct, as I have convinced myself by repeated examinations, more especially from the under side, that the striæ themselves are really beaded."

the objects in view under powers varying from 1,000 to 3,000 diameters, by the aid of the camera lucida.

Scales seen in profile "end on."

Figs. *a*, *b*, *c*, represent the fortunate vertical appearance of a scale of the *Petrobius maritimus*, a Podura frequenting the coast limestone rocks. The scale was luckily caught between the slide and cover during the process of remounting (*a*). A slightly deeper focus showed appearance (*b*), deeper (*c*); a little more pressure caused the scale to turn over and lie nearly flat, recovering its vertical position on the removal of the pressure. This phenomenon solved the problem once for all, that the under side is smooth, the upper ridged. Black beads could be detected in (*c*) at the tips of the ridges. Having secured this fact, attention was next turned to examination of the tips of the spikes with a very exquisite definition of 2,000 diameters. On searching many specimens, a full-grown scale was found and accurately drawn from the microscope by an assistant. Never was a search better rewarded, for not only were the great ribs found beaded on both sides, but up to the very tip (fig. 12*d*); while transversely three or four spherules stretched across, in short rows, apparently supplemented by another parallel set placed in contact in a lower focal plane. Increased pressure being applied the scale was ruptured; the scale oil then flowed out, but returned, insinuating itself along some of its old channels; but while pressed out, the whole molecular structure became visible in the obliterated part; attempted to be shown in the engraving. Besides these a great number appeared to have burst forth from the disruption.

Podura macrotoma.—This little dark-brown leaping insect is very commonly found in the early spring under old timber and brick-bats.

A young scale is represented faithfully in fig. 14, a full grown one in fig. 15, and the cross beading under a power of 3,000 in fig. 16, the ribs being quite out of focus in an upper focal plane. In fig. 15 several spherules have escaped and lie irregularly upon the surface and outside the spikelets, each showing a bright focal point and *annulus*.

It should be here stated that an object-glass was used as condenser of only 15° angular aperture. Large aperture renders the spherules faint and almost invisible. Direct light was employed and *lard oil* with a Seibert 1-32nd, or No. XI objective of excellent quality. Great niceties were also attended to, such as—

(1.) Varying the length of the observing tube between 5 and 16 inches, changing the eye-pieces and especially altering the aberrations by the screw collar.

(2.) Varying the immersion fluid.

(3.) Varying the illumination by means of a series of objectives as condensers having angular apertures between 10° and 85°.

The full and brilliant display of these minute spherules is, therefore, attended with no little patient research. (Direct light used only.)

I wish to call attention to the appearance of a fine full-grown scale of the *Podura macrotoma* under from 2,000 to 3,000 diameters in three different focal planes.

Fig. 18 represents the upper focus.

Fig. 19 represents the middle focal plane.

Fig. 20. The lowest.

The *Podura macrotoma* appears very much to resemble in its scales those of the *Petrobius maritimus*. Younger scales show single rows of spherules on the ribs, and also between them.

Fig. 21. A strongly-marked scale, showing the appearance of large spherules.]

Method of Estimating very Minute Bodies by means of the Micrometer Gauge.

A series of fibres of spun glass is arranged according to thickness, with free ends projecting into the middle of the field of view of the eye-piece, also spider-lines placed across the stop :—

Thickness of	No. 1	accurately	measured	1- 1,750th	of an inch.
,,	No. 2	,,	,,	1- 2,000th	,,
Spider-line	No. 3	,,	,,	1- 6,000th	,,
,,	No. 4	,,	,,	1- 8,000th	,,
,,	No. 5	,,	,,	1-10,000th	,,

If a positive eye-piece be employed magnifying ten times with an objective of 1-10th focal length, the posterior focal image of the glass presents an image magnified 100 times, the total power being 1,000. If, then, No. 1 appears to gauge the size of a magnified object really 100 times less—

(No. 1.) Its size $=\frac{1}{1750}\times\frac{1}{100}=$1-175,000th.

(No. 5.) The size would be $\frac{1}{10000}\times\frac{1}{100}=$1-1,000,000th.

The micrometer guage is not difficult of construction, as the projecting glass fibres can readily be cemented to the stop within the eye-piece. An object is much better compared by bringing it apparently to the free extremity of the fibre, so as to be in juxtaposition.

Examples of Measurement :—1. *Fibrillæ of the H. Janira Moth*: 1-80,000th to 1-90,000th of an inch thick. Black margin of the stem of the fibril about one-fourth of this : thickness of black margin between 1-300,000th and 1-400,000th. (Fig. 13*b*.)

2. *The delicate Membrane uniting the Minute Spikelets of the Scale of the Gnat.*—It may be observed that whenever a membrane is duplicated and flattened a dark black line is formed ; the edge is, in fact, semi-cylindrical, and transmits no light.

If the spider-lines be employed, careful microscopic examination should be made and their actual thickness measured. When they cannot be exposed, a very long focus objective must be employed with a deep eye-piece and a long tube. It should be noted whether they are simple or complex in structure.

Method of Estimating the Size of a Minute Molecule or Line.—Contact is apparently made; first, with the outside of the spider-lines, the micrometer is then read. Inside contacts must now be noted. The mean will be the true size of the molecule, whether the spider webs are of the same thickness or are of different thicknesses.*

If a fine membrane be folded over tightly, the line of flexure appears exceedingly black and straight, and somewhat thicker than the membrane. The folded edge may then be considered at any point as a refracting semi-cylinder, of special optical beauty and precision of form unrivalled in organic life.

To ascertain roughly the nature of this black line, gold beater's skin was folded over and pressed; at the line of flexure a very sharp black line appeared, when examined with a pocket lens. This line appeared about *one-third* thicker than the free edge of the skin forming a section. Gold leaf exhibits similar dark lines of flexure,† upon the malachite green of the gold under transmitted light, if there be but little alloy present. On the whole, this line is thicker than the leaf, or about 1-100,000th, but it is not nearly so sharply displayed as in the flexure of insect scales; in many cases of which the breadth of this black line is even less‡ (particularly in the finest gnat and Podura scales).

Example.—In the gnat's scale the membrane, including the molecules of its structure, stretches in graceful little curves from spikelet

* If R be the first, R′ the second reading δ, δ', the thickness of the webs, it is evident, if Δ be true diameter of molecule,

$$R = \Delta + \tfrac{1}{2}\delta + \tfrac{1}{2}\delta',$$
$$R' = \Delta - \tfrac{1}{2}\delta - \tfrac{1}{2}\delta',$$
$$R + R' = 2\Delta,$$
$$\Delta = \frac{R + R'}{2} \text{ the mean,}$$

which is independent of the size of the webs.

† One grain of gold can be hammered into leaves sufficient to cover 56½ square inches; its thickness will then not exceed 1-282,000th of an inch.—"Encyclopædia Britannica," Article "Gold." The skin is really a tough French paper.

‡ This line has in general resisted every attempt at resolution; yet, when a corrugated scale is folded over, not too sharply, a serrated edge must necessarily exist. I have never been able to define this serration perfectly, except with object-glasses of the highest excellence and reduced aperture, which of course darkened and sharpened the defining shadows. A very fine view was obtained by means of the posterior focus of a 1-50th being shortened to about two inches, which very considerably reduced the aperture and admitted considerable separation of the front lenses, to correct the aberration thus introduced.

to spikelet, and is very black and distinct at the edge with a first-class glass, the objective aperture being reduced by using a condenser of about 15° aperture.

One of these terminators was placed between the spider-lines of a delicate micrometer in such a way that the lined edge was apparently half way between, and the interval on each side between them equal to the thickness of the lined edge or terminating curve. Neglecting the semi-thickness of the spider-lines, the distance between the latter was read 15 millionths (15𝔪); one-third of this is 5𝔪, or the 200,000th of an inch, whilst the general black margins of the ribs appear very much less.

As against transmitted light, causing diffraction, the extremities of the spikelets were barely visible and did not appear one-third the thickness of the black line edge or margin, or about the 600,000th of an inch. Between the ribs forming the spikelets rows of about four molecules could be seen stretching across like the rounds of a ladder. The ribs were 1-50,000th thick; the molecules about one-third of this in diameter.

On one occasion a scale was discovered "end" on. The ribs then appeared rounded off and prominent, and of an intensely blue-black colour; projecting nearly the same amount in relief as they are separated by the corrugated channels between them as semi-cylinders (fig. 22) on one side, *the other being quite smooth.*

The most surprisingly minute lines I have been able to detect were seen in examining an extremely small species of gnat. Mounted fresh and dry, the plumule hairs, about the 1-100,000th thick, were themselves hairy!—ciliated hairs.

The antennæ displayed central grooves also ciliated. And the secretory organ, whence the plume hairs issued, formed a boss or cup, around which a fringe of cilia of very minute magnitude was erected. Comparing these with the familiar images of spider-lines reduced in miniatures 140 times, I should not hesitate to estimate these objects at a size approaching the millionth or microscopical unit (𝔪). An extraordinary instance of accidental magnification of these usually invisible cilia with perfect distinctness, indeed, I might say, with the most vivid definition (probably a power of 10,000 diameters) was discovered to-day. See description of Plates, fig. 17.

DESCRIPTION OF PLATES.

Figures 1–2. Insect scales. Death's head moth under very low and high aperture.

Figure 3. Ordinary appearance of *Degeeria domestica.*

Figure 4. Resolution of *Podura domestica* (same object as fig. 3) into dark and light molecules, 1-50th Powell and Lealand objective.

Figure 5. The approved appearance of the spines of the *Podura domestica* (left half).

Figure 6. Crescentic and tubular shadows. *Degeeria domestica.*

Figure 5*a*. Same object obliquely illuminated, after Mr. McIntyre.

Figures $\alpha, \beta, \gamma \ldots \theta$. Refracting globules of melted glass filaments and refracting spherules. The black annulus varies in thickness, as described according to the conditions of refraction and the degree of angular aperture.

Figure 9 represents the spider-lines in the field of view. Minute black dots appeared on some of the mercurial mirrorlets, but too small for the writer to depict. Where the spider-lines crossed the minute reflecting circlets they appeared intensely black, but otherwise were seen plainly black upon a dark grey ground. The experiment is difficult of successful performance.

Figure 10. Highly magnified Degeeria spherules, with 1-50th object-glass.

Figure 11. Spherules of Degeeria, after Mr. McIntyre. "Monthly Microscopical Journal," 1870, p. 5, vol. iii.

Figures *a*, *b*, *c*. Successive views of spikelets of scale *Petrob. maritimus*, "end on," and in different focal planes, an extremity being one spherule. Plate 4.

Figure 12*d*. Black beaded appearance of spikelets and ribs. Same scale.

Figure 13*a*. Plume of Meadow-brown seen with low aperture, 1-80,000th inch thick being size of the fibrils. (*Hipparchus Janira.*)

Figure 13*b*. The same, with large angular aperture.

Figure 14. Young beaded scale of *Podura macrotoma.*

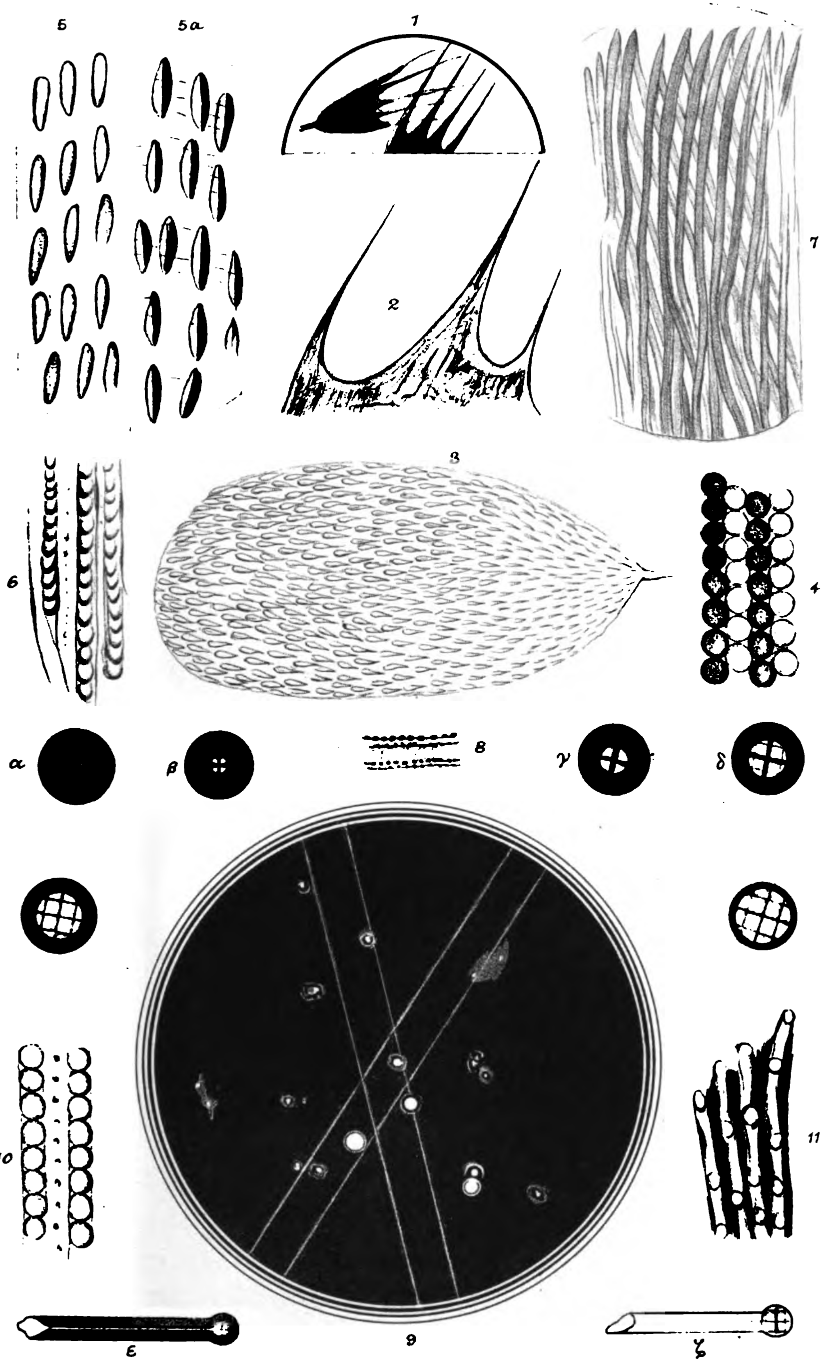
Figure 15. Mature scale; cross rouleaux of molecules developed; loose spherules escaped after pressure.

Figure 16. Highly magnified spikelets and transverse spherules in lower focal plane. Power 3,000, Seibert of Berlin 1-32nd, No. XI objective.

Figure 17. Extraordinary double or compound amplification of ciliary hairs growing on antennæ of male gnat; produced by an irregular oil globule adhering to the stem. The most wonderful power ever seen by the writer, and suggestive of further experiment, the cilia here being otherwise invisible.

Figures 18, 19, 20, and 21. Different appearances of the *Pod. macrot.* in higher, middle, and lower focal planes, 21 being a strongly marked beaded scale.

Figure 22. Gnat scale fortuitously discovered, and one similar to the scale shown, figs. *a*, *b*, *c*, Plate 4. One surface being ribbed, the other perfectly smooth. The intense blue-black is not shown: caused by the arrest of rays of light and the beauty of the definition as in lunar shadows.



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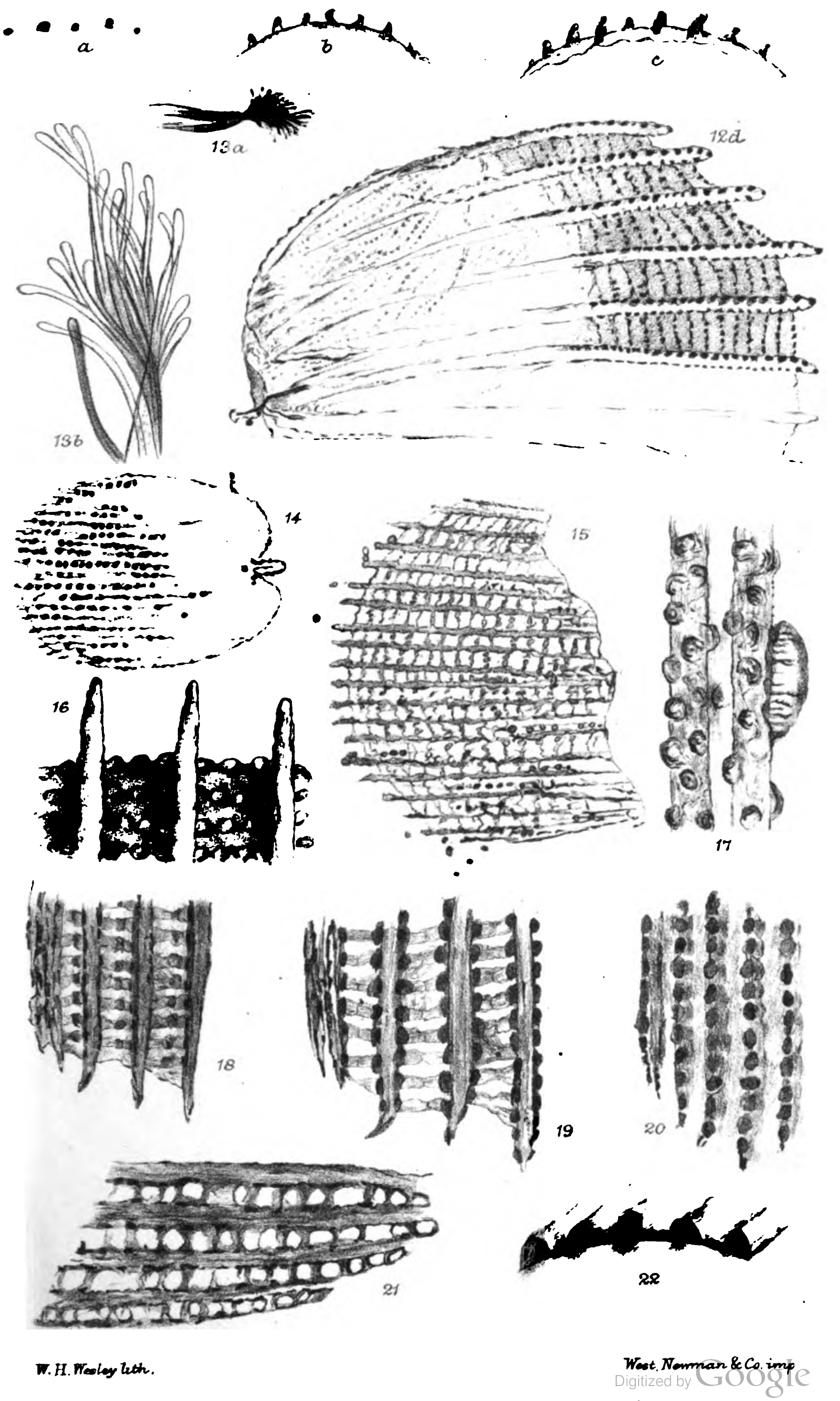

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January 6, 1881.

THE PRESIDENT, followed by General STRACHEY, V.P., in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Researches on the Minute Structure of the Thyroid Gland." By E. CRESSWELL BABER, M.B. Lond. Communicated by E. E. KLEIN, M.D., F.R.S. Received November 25, 1880.

(Abstract.)

This research, which is a continuation of one previously communicated to the Society,* is based on an examination of this organ in the dog, cat, horse, rabbit, ox, sheep, seal, pigeon, fowl, rook, tortoise, frog, skate, and conger-eel. The research has been carried on under the direction of Dr. Klein, and the following are the chief conclusions which have been arrived at :—

(1.) *Vesicles.*—Although branched vesicles may be present, the gland-vesicles in the thyroid of adult animals, according to the author's observations, form closed cavities, which are not in communication with one another to any extent. Much-branched vesicles, which have been observed in the glands of young dogs, are considered merely as forming a stage in the growth of the gland, and as indicating that an increase in the number of vesicles is taking place. In the thyroid gland of the conger-eel, small vesicles (containing colloid) are frequently observed in the walls of large ones, and from the appearances they present it is probable that the walls separating the cavities of the large and small vesicles sometimes become flattened out and give way, allowing the contents of the two vesicles to commingle.

(2.) *Epithelium, Reticulum, and Basement-Membrane.*—Whilst the shape of the epithelial cells of the vesicles varies much in different animals, the general tendency of these cells throughout the vertebrate series, as far as examined, is to the columnar form. A delicate reticulum (as described by Zeiss) has been observed amongst the epithelial cells. In a profile view of the epithelium *club-shaped cells* are also often seen, which are probably indicative of more or less branched cells, situate in the reticulum. The only form of basement-membrane

* "Phil. Trans.," vol. 166, p. 557.

observed has been a delicate layer of connective tissue between the epithelium and the endothelium of the lymphatics, in which the capillaries ramify.

(3.) *Contents of Vesicles.*—The following are the chief constituents of these which have been observed in the microscopical examination of specimens hardened in alcohol:—

(*a.*) *Homogeneous* or *granular material* ("colloid substance" of authors) has been observed with slight variations, in all classes of the vertebrata, and is undoubtedly a normal product of the organ.

(*b.*) *Red Blood-corpuscles* are frequently found in the vesicles in greater or less number, and in various stages of disintegration and decolorisation. In the epithelial wall of vesicles containing red blood-corpuscles granules of yellow pigment are sometimes seen which appear to be due to the absorption, by the epithelial cells or inter-cellular reticulum, of the colouring matter of the effused blood. Observations on the dog lead to the inference that in this animal an escape of red blood-corpuscles is almost constantly taking place into a greater or less number of vesicles, and its occurrence in other animals also (man, tortoise, conger-eel) renders it very probable that the passage of red blood-corpuscles into the vesicles is a normal occurrence in the thyroid gland. Further observations (dog, rook, seal) also make it very probable that normally under certain circumstances an escape of red blood-corpuscles takes place into a large proportion of the vesicles of the thyroid gland. The blood thus effused contributes, the author considers, to a large extent to the formation of the "colloid" substance, and probably has an important bearing on the physiology and pathology of the organ.

(*c.*) *Colourless blood-corpuscles* (large round cells, with granular cell-substance and single round or oval-shaped nucleus) are frequently seen in the homogeneous material in the gland-vesicles of the tortoise. The migration of these cells into the vesicles is a normal occurrence in this animal. It may also take place in other animals.

(*d.*) *Rounded masses* staining darkly with hæmatoxylin, or of a bright yellow colour with picro-carminate of ammonia, are seen in the vesicles of many animals. *Crystals* sometimes occur in the gland-vesicles (tortoise, rook), and finally brown *pigment granules* have been observed in the colloid material contained in the vesicles (frog, conger-eel).

(4.) *Parenchyma.*—The parenchymatous cells already described in the adult dog have been observed in the glands of dogs a few weeks old. In the cat parenchymatous cells have been seen in less numbers, and they appear to migrate into the vesicles in the same manner as in the dog. In the pigeon large groups of round or oval-shaped cells, provided with a single nucleus, are frequently seen, but there is no evidence to show that they migrate into the vesicles.

(5.) *Lymphatics and their Contents.*—A network of lymphatics, resembling, in its general characters, that previously described in the dog, but differing apparently in the extent of its ramifications, has been observed in some other mammals. The homogeneous or granular contents of the lymphatics in the dog occur in both injected and in uninjected glands, and in very variable quantity, the amount being apparently independent of the sex or age of the animal. In the thyroid gland of the pigeon no system of lymphatics could be injected by the method of puncture after repeated attempts, but the injecting fluid always entered the blood-vessels of the gland. The thyroid gland of the tortoise contains a network of lymphatics of which the smaller ramifications run between almost all individual vesicles. The endothelium of these lymphatics presents well-marked areas of varying size and shape, stained darkly with nitrate of silver. In the thyroid gland of the conger-eel no evidence of a system of lymphatics was obtained by interstitial injection.

(6.) *Blood-vessels.*—The arteries are ensheathed more or less completely in their accompanying lymphatics (dog, tortoise). The capillaries, on the contrary, run between the epithelium of the vesicles and the smallest ramifications of the lymphatics (tortoise), leaving intervals between them where the lymphatic is not separated from the epithelium by blood-vessels. Projections of the capillaries between the epithelial cells towards the cavity of the vesicles are frequently seen in the tortoise. In the pigeon the intervesicular ramifications of the blood-vessels are not so complicated nor so minute as in other animals (*e.g.*, tortoise, sheep), but resemble more the distribution of lymphatics in some mammals. In birds also the veins frequently surround either partially or entirely the arteries which they accompany.

(7.) *Undeveloped Portions.*—Bodies consisting of portions of the gland of which the development has not advanced beyond the stage of convoluted "cylinders of cells," described by W. Müller, are frequently seen. They consist of a solid mass of more or less cylindrical rows of cells, between which run septa of connective tissue and blood-vessels. The "cylinders" are composed of cells resembling epithelial cells, cubical or columnar in shape; those on the surface of the cylinder, next to the capillaries, being arranged at right angles to those vessels. In the adult dog these undeveloped portions usually form distinct bodies separated by layers of connective tissue from the rest of the gland, and frequently lying in depressions on the surface of the organ. In young animals, such as the kitten, they can be observed undergoing a transformation into gland-vesicles.

(8.) *Thyroid Gland of the Frog.*—In opposition to the statements of most authors, but in general agreement with those of W. Müller, the thyroid gland of this animal has been found to consist of two distinct bodies, situate one on either side, on the ventral surface of the

hyoid cartilage (or bone) usually, but not always in direct contact with that structure. They are placed either between the hyoid cartilage and the hyoglossus muscles, or else immediately to the outer side of the latter, just anterior to the point of their convergence. Their structure is similar to that of other thyroid glands. The paper is accompanied by eighteen uncoloured and two coloured drawings, illustrating the subjects described.

II. "Note to the Paper on the Structure of the Immature Ovarian Ovum in the Common Fowl and in the Rabbit." By E. A. SCHÄFER, F.R.S. Received November 24, 1880.

Attention was drawn in the above mentioned paper ("Proc. Roy. Soc." vol. 30, p. 239, and fig. 9) to certain peculiar bodies (pseudonuclei) in the vitellus of some immature ovarian ova of the bird, the origin and interpretation of which were left in doubt. In a paper by Rauber,* which I had not seen at the time, the bodies in question are specially noticed, and an account of them is given which agrees closely with mine. I confess, however, I should be slow to acquiesce in the opinion (which Rauber seems disposed to adopt) that they are either directly or indirectly products of immigrated white blood-corpuscles. So far as I can see they appear to be nothing else than condensations of vitelline substance.

I would further take this opportunity of mentioning that the convoluted tube delineated in fig. 35 of the paper referred to in the heading of this note is not to be supposed to represent any part of an egg-tube, but the remains of a tubule derived from the Wolffian body. It is inserted to show the distinction between the two kinds of tubes which may be met with at the same time in the ovary.

III. "Note on a Communication made to the Royal Society by Professor Roscoe, LL.D., F.R.S., 'On the Absence of Potassium in Protagon prepared by Dr. Gamgee.'" By J. L. W. THUDICHUM, M.D., F.R.C.P. Lond. Communicated by JOHN SIMON, C.B., D.C.L., LL.D., V.P.R.S. Received November 25, 1880.

With reference to a communication which Professor Roscoe has recently made to the Royal Society on the subject of "Protagon"

* A. Rauber, "Ueber den Ursprung der Milch und die Ernährung der Frucht im Allgemeinen." Leipzig, 1879.

prepared by Dr. Gamgee, I beg leave to submit the following brief statement.

In order to decide whether "Protagon," as described in 1864 by Oscar Liebreich, is really a definite chemical body, a true immediate principle of the brain, it is of course essential to know whether the substance, prepared as Liebreich directs, does or does not contain other matters than those for which he accounts in his formula of "protagon;" and on this question (or rather on part of it) Professors Gamgee and Roscoe have joined issue with me before the Royal Society.

My contention for some years had been that Liebreich's "protagon" does always contain matters which are foreign to its alleged formula, that it contains them in ponderable quantity, and, among them, notably *potassium*: and a year ago, in my "Annals of Chemical Medicine," I published details of many analyses to show that, in connexion with trifling differences in the extraction-process, the proportion of potassium in different specimens of "protagon" could be made to range from a trace to $1\frac{2}{3}$ per cent.—See *op. cit.*, Art. XIX.

This position of mine Dr. Gamgee sought to refute before the Royal Society by adducing a note from Professor Roscoe to the effect that Professor Roscoe, having examined spectroscopically the carbonised residue of a certain gram of "protagon," had found in it only a quantity of potash which he estimated not to exceed $\frac{1}{20000}$ part of the whole. See "Proc. Roy. Soc.," vol. 30, p. 111.

On this challenge I submitted to the Society that Professor Roscoe's examination had at any rate established as fact the presence of potassium in the "protagon," and that, for reasons which I stated, the quantity of the potassium could not be rightly inferred from any such examination as that which Professor Roscoe had made. See "Proc. Roy. Soc.," vol. 30, p. 278.

Professor Roscoe, apparently in answer to that criticism, has now brought before the Society the result which he obtained in a second examination. Having, in this examination, analysed 5 grms. of "protagon" with regard to the potassium contained in it, he reports that he found it to contain potassium in the proportion of 0·0236 per cent. See "Proc. Roy. Soc.," vol. 30, p. 365.

My reason for asking leave to trouble the Royal Society with the above recapitulation, and with a few words of comment on the present state of the case, is, that Professor Roscoe, in closing his recent communication to the Society, alludes to certain fresh spectroscopic experiments which he has made, and says that they have convinced him of the correctness of the conclusions contained in his original letter. I do not myself quite understand in what sense Professor Roscoe intends that statement, seeing that his later examination of "protagon" had yielded him nearly seven times as much potassium as he had originally

estimated the body to contain. And lest the language now used by him should lead to any misapprehension with regard to the essential issue which has been raised before the Society, I would ask leave to point out that, inasmuch as the specimen of "protagon" which Professor Roscoe analysed yielded him a ponderable quantity of potassium, this result, so far as it goes, thoroughly confirms what I have stated on the subject. In the face of it, none can maintain that "protagon" is the pure chemical body which it claimed to be. Also, adverting to the fact that Professor Roscoe's communication refers to but one analysis, I may perhaps be permitted to express my conviction that, if Professor Roscoe were led to extend his examinations over an area commensurate with mine, I should be indebted to him for further and more decided confirmation of my statements.

Finally, too, I would wish to insist on this contrast: that while "protagon," as I have shown, always on analysis betrays the presence of matters which are foreign to its alleged chemical formula, no such conflict between fact and formula, no so-called "unavoidable impurity," is to be found attaching to those bodies—the kephalines, myelins, lecithins, phrenosins, kerasins, and cerebrinic acid, which I claim as true immediate principles of the brain.

IV. "Preliminary Note on the Existence of Ice and other Bodies in the Solid State at Temperatures far above their ordinary Melting Points." By THOMAS CARNELLEY, D.Sc., Professor of Chemistry in Firth College, Sheffield. Communicated by Professor ROSCOE, F.R.S. Received November 11, 1880.

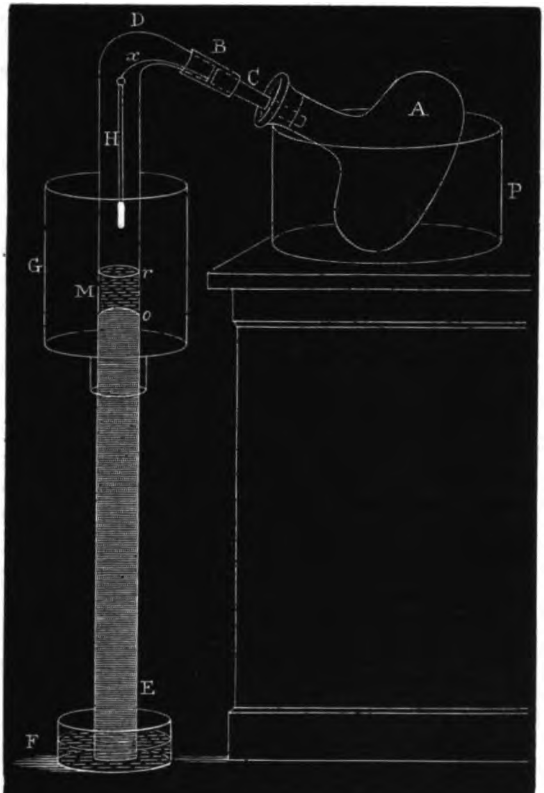
In the present communication I have the honour to lay before the Royal Society a detailed description of experiments, proving that under certain conditions it is possible for ice and other bodies to exist in the solid state at temperatures far above their ordinary melting points. On a future occasion I hope to submit to the Society a full account of the investigation of which these experiments form a part, together with the conclusions to be drawn therefrom. The bodies whose behaviour I propose to discuss at present are ice and mercuric chloride.

Ice.

In the case of ice the great difficulty to be overcome is to maintain the pressure in the containing vessel below 4·6 millims., *i.e.*, the tension of aqueous vapour at the freezing point, for it will be easily understood that if the ice be but slightly heated the quantity of vapour given off would soon be sufficient to raise the pressure above that point. After

several fruitless attempts the following plan, involving the principle of the cryophorus, was adopted.

Fig. 1.



A strong glass bottle, such as is used for freezing water by means of Carré's pump, was fitted with a cork and glass tube C (fig. 1), and the cork well fastened down by copper wire and paraffin wax. A and C were then filled with mercury, and C connected with the end of the tube DE by means of the piece of stout india-rubber pump tubing B, a thermometer having been previously attached by the wire x to the lip of the tube at B. The connexion at B was made tight by fine copper wire. The tube DE was about one inch in diameter, and about four feet long from the bend to the end E; after connexion with C it was completely filled with mercury, care being taken to expel the air from A, C, and DE as completely as possible; the whole was then inverted over the mercurial trough F, as shown in the figure, when the mercury fell to o, the ordinary height of the barometer. The

mercury was run out of A by tilting up the bottle and inclining the tube DE. By this means a Torricellian vacuum was obtained from A to *o*. DE was next brought to the vertical, and the bottle A placed in the trough P. A tin bottle G without a bottom was fitted with a cork, so that it might slide somewhat stiffly along DE.

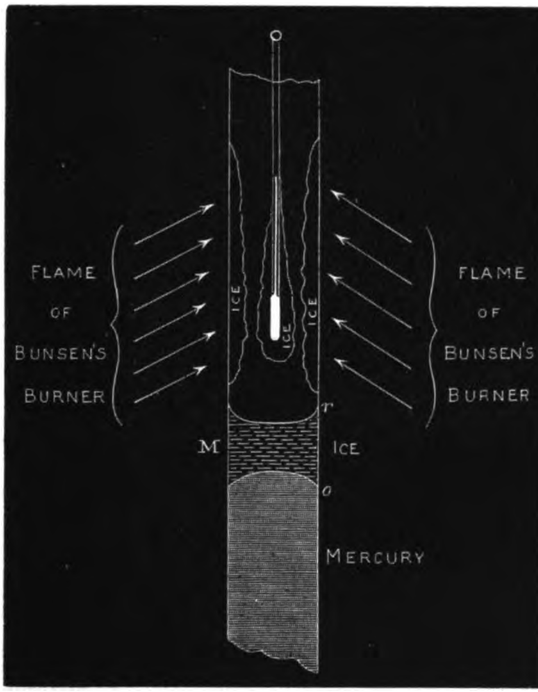
To begin with, the tin bottle was placed in the position G and filled with a freezing mixture of salt and ice. Some boiled water was then passed up the tube DE, sufficient to form a column at M about two inches deep. The thermometer H had been previously arranged, so that its bulb might be one or two inches above the surface of the water M. The bottle A was next surrounded by a good quantity of a freezing mixture of salt and ice, in order that any vapour given off from the water at M might be condensed in A as fast as it was formed, and thus the internal pressure might never be more than about 1·0 to 1·5 millims. When A had been sufficiently cooled, which required about fifteen minutes, the tin vessel G was slid down the tube DE, and its freezing mixture removed. The water at M had then solidified to a mass of ice, which on heating with the flame of a Bunsen's burner melted either wholly or partially, and the liquid formed began at once to boil. The fusion commenced first at the bottom of the column of ice, whereas the upper part fused only with difficulty, and required rather a strong heat. The fusion in this case was probably due to the steam evolved from the lower portions of the ice column being imprisoned and unable to escape, and hence producing pressure sufficient to cause fusion.

When the greater part of the ice had been melted the tube was tightly clasped by the hand, the heat of which was sufficient to produce a somewhat violent ebullition. The liquid in boiling splashed up the side of the tube and on to the bulb of the thermometer, where it froze into a solid mass, as represented in fig. 2. By this means the ice was obtained in moderately thin layers. The tube at the points indicated by the arrows was then strongly heated by the flame of a Bunsen's burner, with the following results:—

The ice attached to the sides of the tube at first slightly fused, because the steam evolved from the surface of the ice next the glass being imprisoned between the latter and the overlying strata of ice, could not escape, and hence produced pressure sufficient to cause fusion, but as soon as a vent-hole had been made fusion ceased, and the whole remained in the solid state, and neither the ice on the sides of the tube nor that on the bulb of the thermometer could be melted, however great the heat applied, the ice merely volatilising without previous melting. The thermometer rose to temperatures varying between 120° and 180° in different experiments, when the ice had either wholly volatilised or had become detached from the bulb of the thermometer. The ice attached to the latter did not partially fuse at the

commencement of the heating because, the heat reaching the outer surface of the ice first, evaporation could take place from a free surface and the vapour not become imprisoned, as was the case with the ice attached to the sides of the tube.

Fig. 2.

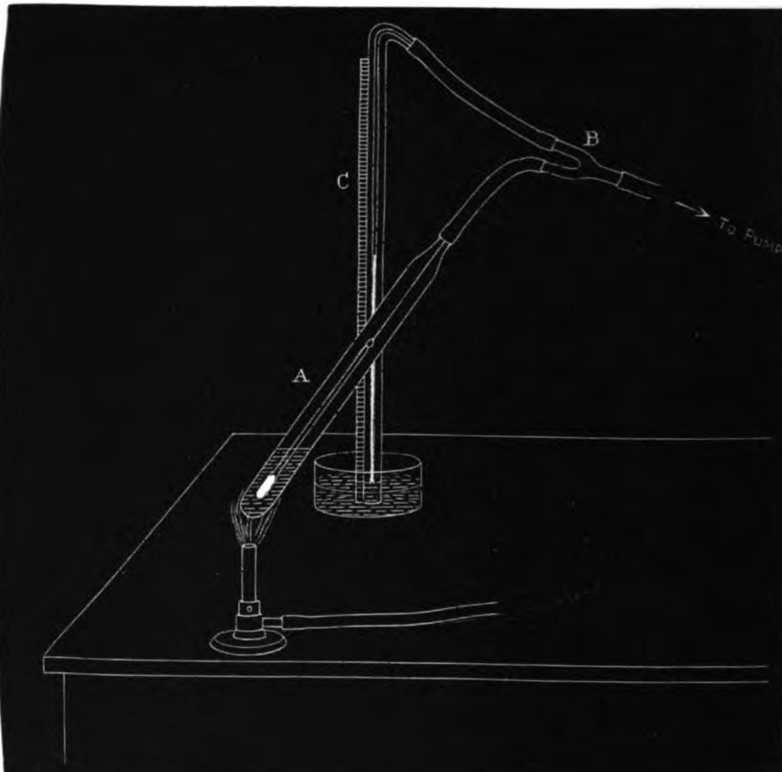


These experiments were repeated many times and always with the same result, except in one case in which the heat applied had been very strong indeed and the ice attached to the sides of the tube fused completely. On removing the lamp, however, for a few seconds the water froze again, notwithstanding that the portion of the glass in contact with it was so hot that it could not be touched without burning the hand.

The chief conditions necessary for success appear to be—(1) That the condenser (A, fig. 1) is sufficiently large to *maintain* a good vacuum. In the present case the capacity was about three-quarters of a litre; (2) That the ice is not in too great mass, but arranged in thin layers. Further, in the case where the heat is applied to the under surface of the layers of ice, the latter must be sufficiently thin to allow of a vent-hole being formed for the escape of the steam coming from

below, otherwise fusion occurs. When the heat is applied to the free surface of the ice, the layers may be much thicker.

FIG. 3.



Mercuric Chloride,

m. p.=288°, re-solidifies at 270—275°, b. p.=303°.

About 40 grs. of pure mercuric chloride were placed in the tube (A, fig. 3), and a thermometer arranged with its bulb imbedded in the salt. The drawn-out end of the tube was connected by stout india-rubber tubing with one branch of the three-wayed tube B, whilst the other was attached to the manometer C. B was connected with a Sprengel pump, fitted with an arrangement for regulating the pressure.

When the pressure had been reduced by means of the pump to below 420 millims., the mercuric chloride was strongly heated by the flame of a Bunsen's burner, with the following results:—

Not the slightest fusion occurred, but the salt rapidly sublimed into

the cooler parts of the tube, whilst the unvolatilised portion of the salt shrank away from the sides of the tube and clung tenaciously in the form of a solid mass to the bulb of the thermometer, which rose considerably above 300° C., the mercury of the thermometer shooting up to the top of the stem. After slight cooling the air was let in, and under the increased pressure thus produced the salt attached to the bulb of the thermometer at once melted and began to boil, cracking the tube at the same time.

The experiment was next varied as follows:—

About the same quantity of chloride was placed in the tube A, fig. 3, as before, and heated by the full flame of a Bunsen's burner. The lamp was applied during the whole of this experiment, and the size of the flame kept constant throughout. The mercuric chloride first liquefied and then boiled at 303° under ordinary pressure, and whilst the salt was still boiling the pressure was gradually reduced to 420 millims., when the boiling point slowly fell to 275°, at which point the mercuric chloride suddenly began to solidify, and at 270° was completely solid, the pressure then being 376 millims. When solidification was complete the pump was stopped working, but the heat still continued to the same extent as before. The salt then rose rapidly to temperatures above that at which a thermometer could be used, but not the least sign of fusion was observed. From the completion of the solidification to the end of the experiment the pressure remained at about 350 millims.

The above experiment, which was repeated three times, shows, therefore, that when the pressure is gradually reduced from the ordinary pressure of the atmosphere to 420 millims., and the boiling point simultaneously from 303° to 275°, the salt solidifies while it is still boiling and in contact with its own hot liquid, notwithstanding that it is being strongly heated at the same time; and that, after solidification is complete at 270°, the temperature then rises far above the ordinary boiling point (303°) of the substance without producing any signs of fusion. Under ordinary circumstances, mercuric chloride melts at 288° and re-solidifies at 270—275°, *i.e.*, at a temperature identical with that at which it solidifies under diminished pressure, as above described.

The solid mercuric chloride obtained on solidification under the combined influence of diminished pressure, and the application of a strong heat had a peculiar appearance, quite different to that produced when the substance is allowed to solidify in the ordinary way. It appeared to consist of a mass of pearly leaflets closely packed together round the bulb of the thermometer.

Any final explanation of these phenomena is reserved until further experiments have been made.

Received December 16, 1880.

Appendix.

Since writing the foregoing, it has been said in explanation of the phenomena therein described, that the thermometer, though embedded in the mass of the ice, did not really indicate the true temperature of the latter. With the object, therefore, of proving whether the ice is hot or not, I have, at the suggestion of Professor Roscoe, made the following calorimetrical determination:—

The arrangement of the apparatus was so modified, that the ice, after being strongly heated, could be suddenly dropped into a calorimeter containing a known quantity of water of known temperature. The resulting temperature, after the ice had been dropped in, was read off by a thermometer graduated so as to indicate a difference of $0^{\circ}\cdot05$ C. The weight of the ice was found by re-weighing the calorimeter.

So far, I have only had the opportunity of completing the two following determinations, and in the second of these the weight of the ice could not be found, as a small quantity of water was lost out of the calorimeter, owing to a sudden jerk at the moment the ice entered it:—

(1.) Weight of water in calorimeter, including the value of the latter=185 grms.

Weight of ice dropped in=1·3 grms.

Temperature of calorimeter before	=$13^{\circ}\cdot4$
" " after	=13·6
Rise in temperature	= 0·2

$$M(\theta-t) \quad + \quad 80W \quad = W(T-\theta)$$

$$(185\times 0{\cdot}2)+(80\times 1{\cdot}3)=1{\cdot}3(T-13{\cdot}6)$$

$$\therefore \quad T=122^{\circ}\text{ C.} \quad \text{Where } T=\text{temperature of ice.}$$

(2.) Weight of water in calorimeter, &c.=185 grms.

Temperature of calorimeter before	=$12^{\circ}\cdot7$
" " after	=12·8
Rise in temperature	= 0·1

On weighing the calorimeter after the experiment, the increase in weight was only 0·15 grm., but as a portion of the water had been jerked out during the operation, the true weight of the ice, and there-

fore its temperature, could not be found. But since the calorimeter had slightly risen in temperature, the ice must have been above 80° C.

From the nature of the experiment, as carried out on the present scale, the weight of the ice which can be dropped into the calorimeter is only small, and therefore the rise in temperature is but slight. But since a fall in temperature of a much larger amount ought to have been obtained had the ice been at 0°, it is considered that the above experiments are conclusive. Great care was taken, in order to obtain correct temperatures in the calorimeter. The latter was inclosed in several casings, and the water was allowed to stand in it for several hours before the experiment, so that it might first attain the temperature of the room, whilst the time which elapsed between the readings of the thermometer before and after the ice was dropped in would not be more than from 10 to 15 seconds.

In the course of the next few weeks I intend to make one or two more determinations, and, if possible, on a larger scale.

V. "On the Effects of Heat on the Chloride, Bromide, and Iodide of Silver, and on some Chlorobromiodides of Silver." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor TYNDALL, F.R.S. Received November 26, 1880.

1. *The Chloride, Bromide, and Iodide of Silver.*

Since I had the honour of submitting to the Society papers on the above subjects, "Proc. Roy. Soc.," vol. 25, p. 279—303, accurate determinations, by a new method, of the melting points of the substances with which the experiments were made have been described by Mr. Carnelley. He finds that the melting point of iodide of silver, hitherto generally described as "a low red heat," is 527° C.; of the bromide of silver 427° C.; and of the chloride 451° C. These numbers are in all cases higher than those which I adopted, and I have, consequently, recalculated those portions of the results which are affected by the new determinations, and they are given in the following table. The volume of the chloride and bromide at − 60° C. has been taken as unity for better comparison with the iodide, which, according to Fizeau, possesses its maximum volume at that temperature. It will be remembered that the iodide of silver in *cooling* from its melting point contracts like ordinary solids until it reaches a temperature of 142° C., at which point it possesses its maximum density, it then suddenly expands to a volume greater than that which it possesses at the fusing point, and continues to expand as the temperature diminishes.

	Chloride of silver.	Bromide of silver.	Iodide of silver.
Fusing point	451° C.	427° C.	527° C.
Specific gravity at 0° C.	5·505	6·245	5·675
„ „ fusing point	4·919	5·595	5·522
Volume at −60° C.	1·000000	1·000000	1·017394 Maximum volume.
„ −10° C.	.. ..	.. ..	1·017342
„ 0° C.	1·005547	1·006060	
„ 70° C.	.. ..	.. ..	1·017009
„ 100° C.	1·015092	1·016560	
„ 142° C.	.. ..	.. ..	1·015750 After sudden expansion. 1·000000 Maximum density.
„ 200° C.	1·024937	1·027460	1·001649
„ 300° C.	1·035082	1·038760	1·004493
„ 400° C.	1·045227	1·050460	1·007337
„ fusing point (solid)	1·050819	1·053470	1·010940
„ „ „ (liquid)	1·116427	1·122840	1·044990
Physical structure, &c., of fused mass	Crystalline fracture; thin layers transparent and flexible; thick rods very flexible when hot.	Crystalline and brittle.	Transparent and plastic above 142° C.; crystalline, opaque, and brittle below 142° C.

2. *The Chloro-brom-iodides of Silver.*

Mr. Carnelley has been so good as to determine for me, by his new method, the melting points of the five chloro-brom-iodides of silver described in my former paper ("Proc. Roy. Soc.," vol. 25, p. 292), and the results have been recalculated in accordance therewith. By reference to the accompanying table, it will be seen that the fusing point of No. 1 is 44° C. lower than that of its most easily fusible constituent, and 144° C. lower than that of the least easily fusible constituent. The lowering is most conspicuous in No. 3, the fusing point being 101° C. lower than that of bromide of silver, which constitutes nearly one-quarter of its weight, and 201° C. lower than that of iodide of silver, which constitutes more than half its weight. Again, in No. 4 it will be seen that the melting point is 147° C. lower than of iodide of silver, which constitutes nearly three-fourths of its weight.

We may notice, moreover, the following points:—

(1.) No. 1, containing the smallest quantity of iodide of silver, is almost unaffected by it. It closely resembles bromide of silver, save that a very slight contraction takes place between 125°·5 C. and 131°·5 C.; and the orange-coloured powder furnished by pulverisation turns green on exposure to light. In No. 3, which contains a little more than half its weight of iodide of silver, we find the lowest melting point; the highest specific gravity; the greatest divergence in every respect from the properties of its constituents, although the influence of the iodide is very marked, both in the plasticity of the substance above 250° C., and in the considerable contraction which takes place between 124° C. and 133° C. In No. 5 the influence of the iodide is most marked, and in some respects the substance resembles its principal constituent.

(2.) Let us note, moreover, that the fusing points diminish from 1 to 3, and increase from 3 to 5; while the specific gravities diminish on both sides of No. 3.

(3.) In No. 1, in spite of the presence of the iodide, the coefficients of expansion above 131·5 and below 125·5 are higher than those of any of its constituents. Again, in spite of the presence of such expansible bodies as the chloride and bromide of silver, the amount of contraction undergone by some of the chloro-brom-iodides, on heating between 124° C. and 133° C., exceeds that of the iodide itself. The case becomes one of great complexity. Take the most distinctive of the chloro-brom-iodides, No. 3. Between 0° C. and 124° C., any mass of a hundred molecules consists of 58 which are undergoing slight contraction, and 42 which are undergoing rapid expansion. Rapid contraction takes place with the iodide alone at 142° C.; here it commences 18° C. lower, and while it takes place in the case of 58

	1. AgI, Ag_2Br_2, Ag_2Cl_2.	2. $AgI, AgBr, AgCl$.	3. $Ag_2I_2, AgBr, AgCl$.	4. $Ag_3I_3, AgBr, AgCl$.	5. $Ag_4I_4, AgBr, AgCl$.
Composition in 100 parts :—					
Iodide of silver	26·1692	41·484	58·6404	68·0171	73·9285
Bromide of silver	41·8708	33·186	23·4557	18·1379	14·7856
Chloride "	31·9600	25·330	17·9039	13·8450	11·2859
Fusing point	383° C.	331° C.	326° C.	354° C.	380° C.
Specific gravity at 0° C.	6·152	6·1197	6·503	5·9717	5·907
" " fusing point	5·5118	5·5673	5·6971	5·6430	5·680
" " 0° C. calculated on the assumption that no change of volume occurs.	5·836	5·8010	5·762	5·741	5·725
Volume at 0° C.	1·000000	1·000000	1·000000	1·000000	1·000000
" 124° C.	1·015331*	1·012037	1·010301	1·007440	1·006696
" 133° C.	1·015037	1·006637	·993201	·984041	·979696
Volume of solid at fusing point	1·054986	1·046646	1·032283	1·009612	1·006372
" liquid at solidification point.	1·112376	1·097486	1·059998	1·057645	1·040513
Certain physical properties	Crystalline fracture; brittle both when hot and cold. Resembled bromide of silver.	Compact, hard, homogeneous; semi-crystalline fracture, very tenacious when cold; bends slightly at 250° C., but breaks easily.	Compact, hard, homogeneous, very tenacious when cold; at 250° C. flexible enough to be bent through an angle of 40° without breaking.	Brittle when cold; at 250° C. flexible enough to be bent through a right angle.	Crystalline fracture, more brittle when cold than any of the preceding, except No. 1. Very flexible at 250° C., and capable of being twisted without fracture.

* In the case of No. 1, the temperatures at which contraction began and finished respectively were 125°·5 and 131°·5, not, as in the case of the others, 124° and 133° C.

molecules, the 42 molecules of bromide and chloride are still expanding. Finally, from 133° C. to the fusing point, all the molecules are expanding, 42 of them quickly and 58 slowly. And during this heating from 0° C. to 133° C., a highly crystalline brittle solid has been converted, within the mass of two crystalline solids, into a plastic amorphous body.

VI. "Phenomena of the Capillary Electroscope." By G. Gore, LL.D., F.R.S. Received November 23, 1880.

(Abstract.)

In this communication is described an investigation of various of the conditions of the above phenomena. The phenomena have been found to be purely physical except in those cases where the electricity was of too high tension and produced electrolysis, and in those in which the solution acted chemically upon the mercury.

The influence of the kind and strength of numerous solutions was examined, including those of various salts of neutral, alkaline, and acid reaction, and of various acids, and the results are described. Of the salts, some gave anomalous results; alkaline cyanides, for instance, reversed the direction of the movements of the mercury. Inferior conducting liquids, such as aqueous ammonia, gave feeble movements. Good conducting solutions, such as neutral salts of the alkalies, potassic cyanide, and dilute sulphuric and phosphoric acids, yielded strong movements. Both the kind and the degree of dilution of the various substances had great influence upon the results.

By examining the question of the influence of the relative dimensions of the mercurial surfaces it was found that no such influence existed, simply because one surface of liquid metal with a suitable solution was alone sufficient.

Attempts were made, but without success, to obtain the same movements of two aqueous solutions instead of mercury and a watery liquid. Other effects, of apparent movement, &c. (described in a separate communication, see "Influence of Voltaic Currents on the Diffusion of Liquids," (*ante*, p. 250), were obtained.

The question as to whether the movements were wholly due to alteration of volume of the mercury was also investigated, and the conclusion arrived at was, that while a very minute alteration of volume did occur, nearly the whole of the movement was due to other causes.

The influence of adhesion was freely examined, and the hypothesis proposed that the motion is primarily due to a direct mechanical action at the immediate surfaces of contact of the mercury and solution,

consisting of either mutual attraction or repulsion of the two liquids, and that the to and fro movement is a secondary circumstance arising from this in consequence of the greater adhesion of the solution than of the mercury to the tube.

The relations of the phenomena to heat and temperature are discussed, and the inference drawn that these influences perform only an unimportant part in the actions.

With regard to the electric conditions, it is observed that the phenomena are inseparably connected with electric transfer, and may be attended or not by electrolysis; the latter, when present, being only a coincidence. Various experiments were made with electricity derived from an electrophorus, and the effects obtained are described in different parts of the paper; and the sensitiveness of the instrument to electric influence is illustrated. From the various results obtained by the author and other investigators, a conclusion is drawn that the phenomenon is essentially one of electric convection; that the primary mechanical movement at the two liquid surfaces is due to a more or less charged electric state of those surfaces, which under the conditions of unequal adhesion of the two liquids to the tube produces the to and fro movement.

The movements were independent of chemical action.

VII. "Electric Currents caused by Liquid Diffusion and Osmose." By G. Gore, LL.D., F.R.S. Received December 9, 1880.

The following experiments arose out of a research on "The Influence of Voltaic Currents on the Diffusion of Liquids." As the converse phenomena of electric diffusion and electric osmose of liquids would be the production of electric currents by liquid diffusion and osmose, I made various attempts to obtain them.

Other investigators have already obtained electric currents by means of the contact of two liquids.* Nobili appears to have been the first. In his arrangement, each element consisted of a series of four glass cups, each containing an electrolyte, the liquids in the two terminal vessels being precisely alike, and containing platinum electrodes, and a second and third kind of liquid being in the two intermediate cups respectively. Liquids of suitable specific gravity were selected, such as not to spontaneously mix. The solutions were connected by means of liquids (similar to those in the cups) contained

* Nobili, "Ann. de Chem. et Phys.," vol. xxxviii, 1828, p. 239; "Pogg. Ann.," vol. xiv, 1828, p. 169; Fechner, "Pogg. Ann.," vol. xlviii, 1839, pp. 1 to 225; Wild, "Pogg. Ann.," vol. ciii, 1858, p. 353; Schmidt, "Pogg. Ann.," vol. cix, 1860, p. 106; Wüllner, "Pogg. Ann.," vol. cvi, 1859, p. 454, vol. cix, 1860, p. 94.

in glass siphons having turned-up capillary ends. As, in his arrangement, acid and alkaline liquids were in contact with each other, chemical action occurred. Fechner employed a similar apparatus, and in some of the combinations of liquids employed by him, chemical action took place from a similar cause. In Wild's research, each element consisted of an open cup having two long and vertical wide glass tubes projecting downwards from its lower surface and terminated at their lower ends by electrodes. Three kinds of liquid, in four portions, of suitable specific gravities, were carefully placed upon each other in a series of distinct layers, without intervening solid diaphragms, the two portions in contact with the electrodes being precisely similar. Liquids which did not chemically act upon each other were in nearly all cases employed; but as the electrodes were electro-coppered metallic capsules, and any electric current produced by liquid diffusion has extremely feeble electromotive force, it is probable that chemical action interfered. L. Schmidt (of Halle) employed a perfectly similar apparatus and arrangement of liquids. In Wüllner's arrangement, the liquids were in a wide cylinder formed of several horizontal segments, the liquids being in distinct layers, separated by disks of animal membrane, and the electrodes were circular sheets of brass or copper.* In each and all of these apparatuses and arrangements, three distinct liquids were employed, and the phenomena were not reduced to their simplest form by the use of two liquids only.

In my earliest experiments, I employed several different arrangements of apparatus, in which a very limited number of layers of liquid of different degrees of concentration were separated from each other by horizontal diaphragms of parchment paper, the terminal layers having small platinum wire electrodes connected with a Thomson's reflecting galvanometer. No signs of an electric current were however obtained.

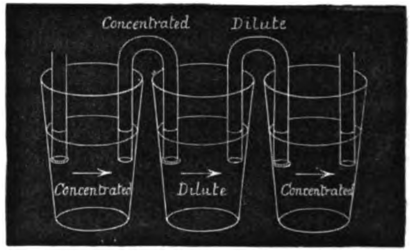
As electromotive force may be of all degrees of strength, from that which only causes two liquids to feebly mix to that by which the most fixed compounds are decomposed; and as the energy of mechanical diffusion and osmose is extremely feeble in comparison with that of chemical union, I concluded that any electric current arising from those causes would probably have very feeble electromotive power, and I therefore made other trials with a larger number of elements.

In order to try more effectually whether a difference of facility of diffusion, caused by the action of gravity upon two portions of solution of different degrees of concentration and of specific gravity, would produce an electric current, I made the following experiments:—

Experiment 1.—I took a series of twenty-five small glass tumblers, as

* See also Kamtz, "Schweigg. Jnl.," 1829, vol. lv, p. 1; Lagarve, "Gilb. Ann.," vol. xiv, 1803, p. 230; Kohlrausch, "Pogg. Ann.," vol. lxxix, 1850, p. 200; Worm Müller, "Pogg. Ann.," vol. cxl.

Fig. 1.



hown in fig. 1, each alternate one containing a liquid of a different degree of concentration. Two liquids were employed, viz., one composed of a half saturated solution of potassic nitrate, and the other, one volume of a saturated solution of the same salt, mixed with 320 volumes of water. Each alternate vessel contained respectively 3 ounces by measure of the weaker, and $2\frac{1}{2}$ oz. of the stronger solution. The two terminal vessels contained the stronger liquid, and were provided with small platinum wire electrodes inserted through holes near the bottoms of the vessels. The liquids in the vessels were connected in single series by means of inverted bent tubes of glass, of half-an-inch diameter, the alternate tubes being filled with the respective solutions. The end of each of the tubes at which the two liquids came into contact, was previously covered with a wet septum of parchment paper tightly secured by means of string. On connecting the series with the galvanometer, a feeble electric current was obtained, which by interchanging the two terminal vessels was proved not to arise from any differences of the electrodes. The direction of the current was downwards through the septums and surfaces of contact of the two liquids.

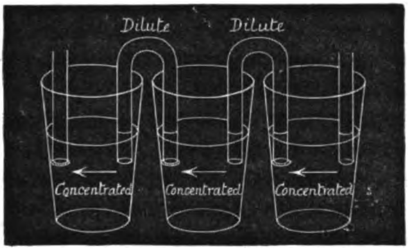
As according to statements in text books on physics a solution of potassic nitrate is one in which diffusion is not increased and which is not decomposed by a diaphragm, I conclude from the above results that the electric current obtained was probably due to greater rapidity of diffusion at the alternate septums where the heavier liquid was above than at the others.

Experiment 2.—In order to ascertain whether an electric current would be similarly produced by a different kind of liquid, I took a series of thirty-five such elements. The liquids consisted of a strong aqueous solution of cupric sulphate freely acidulated with sulphuric acid, and a mixture of $1\frac{1}{4}$ drachm of the same solution with 65 oz. of water. A somewhat stronger electric current, similar in direction, was obtained, and probably arose from a similar cause to that in the previous experiment.

Experiment 3.—In order to determine whether an alternation of osmose and diffusion of the two liquids obtained by the presence of a

diaphragm and no diaphragm would also give rise to an electric current, I charged the whole of the thirty-five glass cups with the acidulated strong solution of cupric sulphate employed in the last experiment, and filled all the connecting tubes with the weaker solution used in that experiment, taking care to alternate the ends of the tubes which were open with those which were closed by a septum, as in the annexed sketch (fig. 2). I also employed as electrodes, moveable

Fig. 2.



platinum wire of much larger surface, cut from contiguous portions of the same piece of wire and insulated at their upper parts from the liquid by means of glass tubing. By this arrangement a much stronger electric current was produced than in the previous experiments; but in a reverse direction, viz., from the strong solution upwards through the diaphragm into the weak one, and from the weak one down through the meniscus into the strong one.

Experiment 4.—A similar experiment to No. 3 was now made with a nearly saturated solution of potassic nitrate and a mixture of half a drachm of that solution with 34 oz. of water. The current was the same in direction as in the last experiment, but more feeble; and was strongest at the first contact.

Experiments 5 *and* 6.—These experiments were similar to No. 4, except that (with the hope of obtaining a stronger effect) the whole of the bent tubes were filled with the solution previous to placing their ends in the vessels;* the strengths of the solution of saltpetre were also somewhat different. The currents obtained were the same in direction and magnitude as in Experiment No. 4.

Remarks.—In all these four last experiments, the electric currents produced by the difference of osmose caused by a porous partition, were upwards through the diaphragms and opposite in direction to those produced in Experiments Nos. 1 and 2 by difference of diffusion.

I have since obtained very much stronger currents, sufficient to readily send the image off the scale, by means of even a single diffusio-electric element.

* Henceforth the whole of the tubes were always filled previous to immersion.

VIII. "Additional Note to a Paper 'On the Thermal Conductivity of Water.'"* By J. T. BOTTOMLEY, Lecturer in Natural Philosophy, and Demonstrator in Experimental Physics in the Uinversity of Glasgow. Communicated by Professor G. G. STOKES, Sec. R.S. Received December 21, 1880.

(Abstract.)

In the former paper, the author had supposed that by taking a vessel of very large horizontal section he should be able to eliminate the sensible effects of conduction of heat downwards by the sides of the vessel. However, considering the important part which the sides might conceivably play, and the statements in books and published writings that the conduction of heat downwards in liquids in the experiments of Despretz and others, was wholly, or almost wholly, due to conduction by the sides of the containing vessel, it was thought advisable to put the matter to a direct test.

A screen of thick cotton material was accordingly placed inside the cylindrical vessel used in the experiment. The screen extended all round, at a little distance from the side, and reached from top to bottom, and accordingly would arrest currents of convection arising from heat conducted down the side, if such were sensible.

Experiments made in March and April, 1880, gave for the conductivity of heat by water, in square centims. per second, a mean of 0·00134 without the screen, and 0·00154 with the screen. These numbers, though not in complete agreement, are yet quite of the same order. They indicate that no important error is to be feared as the result of conduction down the walls of the bath. Indeed, the apparent conducting power of water comes out a little *greater* with the screen than without.

* For an abstract of the paper see "Proc. Roy. Soc.," vol. 28, p. 462.

January 13, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Hon. Sir George Jessel, Master of the Rolls, was admitted into the Society.

The Right Hon. William Ewart Gladstone, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On the 48 Co-ordinates of a Cubic Curve in Space." By WILLIAM SPOTTISWOODE, President R.S. Received December 29, 1880.

(Abstract.)

In a note published in the Report of the British Association for 1878 (Dublin), and in a fuller paper in the "Transactions of the London Mathematical Society," 1879 (vol. x, No. 152), I have given the forms of the eighteen, or the twenty-one (as there explained), co-ordinates of a conic in space, corresponding, so far as correspondence subsists, with the six co-ordinates of a straight line in space. And in the same papers I have established the identical relations between these co-ordinates, whereby the number of independent quantities is reduced to eight, as it should be. In both cases, viz., the straight line and the cubic, the co-ordinates are to be obtained by eliminating the variables in turn from the two equations representing the line or the conic, and are, in fact, the coefficients of the equations resulting from the eliminations.

In the present paper I have followed the same procedure for the case of a cubic curve in space. Such a curve may, as is well known, be regarded as the intersection of two quadric surfaces having a generating line in common; and the result of the elimination of any one of the variables from two quadric equations satisfying this condition is of the third degree. The number of coefficients so arising is $4\times 10=40$; but I have found that these forty quantities may very conveniently be replaced by forty-eight others, which are henceforward considered as the co-ordinates of the cubic curve in space. The relation between the forty and the forty-eight co-ordinates is as follows:—

On examining the equations resulting from the eliminations of the variables, it turns out that they can be rationally transformed into expressions such as $UP'-U'P=0$, where U and U′ are quadrics, and P and P′ linear functions of the variables remaining after the eliminations. The forty-eight co-ordinates then consist of the twenty-four coefficients of the four functions of the form U (say the U-co-ordinates), together with the twenty-four coefficients of the functions of the form U′ (say the U′-co-ordinates), arising from the four eliminations respectively: *viz.*, $4\times 6+4\times 6=48$. And it will be found that the coefficients of the forms P, P′, are already comprised among those of U, U′; so that they do not add to the previous total of forty-eight.

The number of identical relations established in the present paper is thirty-four. But it will be observed that the equations $UP'-U'P=0$ are lineo-linear in the U-co-ordinates and in the U′-co-ordinates; and as we are concerned with the ratios only of the coefficients, and not with their absolute values, we are, in fact, concerned only with the ratios of the U-co-ordinates *inter se*, and of the U′-co-ordinates *inter se*, and not with their absolute values. Hence the number of independent co-ordinates will be reduced to $48-34-2=12$, as it should be.

The thirty-four identical relations arrange themselves firstly in two sets: one set belonging wholly or principally to the U-co-ordinates, and the other set wholly or principally to the U′-co-ordinates. In each set there are four groups: one of four, one of eight, one single, and again one of four equations; seventeen in all. In the course of the paper, the two groups of eight are obtained in two forms: first, by a purely algebraical method in a rational form; and secondly by a method partly geometrical and partly algebraical, in an irrational form.

II. "How do the Colour-blind See the different Colours? Introductory Remarks." By Frithiof Holmgren, Professor of Physiology, University, Upsala. Communicated by W. Pole, Mus. Doc., F.R.S. Received December 6, 1880.

That the colour-blind do not see colours in the same way with normal-eyed persons we may know from the fact that they confuse rays of objective light which, to the normal eye, give quite different impressions.

When, for instance, a red-blind person is confused in his perception of those different sorts of light that to the normal eye appear as red and green, we may conclude that he sees them both as one and the same colour, but not what that colour is, as to its quality—whether it is one of those just mentioned or a third—and whether, on the latter supposition, that colour exists in the colour-system of normal-eyed

persons, say as yellow, or, on the other hand, is a colour of which we in general have not the slightest conception.

If a theory of colour-blindness has for its object to explain the different links in the chain of causes and effects, of which the first is the objective light, and the last the subjective perception thereof, we must first know this last.

From the points of view of the different theories, different opinions have been entertained on this subject, but practical proofs have hitherto been wanting for them all, and, what is worse, the hope of ever gaining a solid basis for such proofs has been given up, for the reason that they must be found in the subjective perception of another man.

Since it is impossible for one person to make himself master of that conception in another, we cannot even objectively prove that all normal-eyed persons see the different colours in the same way. Still it may be assumed as an axiom that at least the quality of the different principal colours is the same for all persons who show the same conception of colours in general. Else we should not talk of colour, as all spiritual communication would be impossible between persons whose sensuous impressions were quite at variance.

If that axiom is accepted, and we take for granted that all normal-eyed persons see colours in the same way, as do also the colour-blind of the different sorts in their own particular manner, I will endeavour to show that the question which stands at the head of this article is not only not impossible to answer, but that I can already give that answer, furnished with scientific and objectively binding proofs.

There is apparently but one way to establish such an answer. A normal-eyed person must, while retaining his own colour-sense, be put in position to see with a colour-blind eye, and compare the impressions on that with those on his own normal eye. He cannot use either the one or the other unless they stand in a living organic communication with his own brain. He must, so to speak, grow into the colour-blind and his chromatic sense without losing the use of his own.

Such a phenomenon, which here is a *conditio sine quâ non*, it is, of course, impossible to procure in an artificial way; but still I will show that an organic combination of a normal and a colour-blind eye with the same brain is not impossible to be realised in a perfectly natural manner.

Congenital colour-blindness is, as is known, in most cases inherited, and inherited according to definite laws. (*Vide* Frithiof Holmgren: "La Cécité des Couleurs en Suède," p. XIII.) One of these laws is, that all the children of the same parents are not afflicted with this defect; some are free. Nor is the colour-blindness of the same degree in those affected. Accordingly it would seem not to be contrary to the laws of inheritance that a person should be born with one normal eye, while the

other was more or less colour-blind, or that while he was colour-blind with one eye, the other should be imperfectly so, perhaps in so slight a manner that it might be called nearly normal.

Besides the congenital defect, there exists an acquired one, which does not necessarily affect both eyes at the same time.

A combination of a normal and an abnormal eye with the same brain is, as we see, not impossible. A few such cases have been noticed in the literature of the subject as curious exceptions, one recently by Professor Becker, in Heidelberg, which is described by him. Such a case is what is wanted: a colour-blind person who can make his conception of the different colours subjectively clear for a normal-eyed person, as well as this latter can make the same conception objectively clear to all other people with normal sight. We have thus a bridge between the subjective perception of the colour-blind person and objective scientific research.

Led by this idea, I have during the last three years looked for cases of this one-sided colour-blindness in combination with my statistical researches through Sweden. The difficulties that formerly were so enormous, and which have been removed by my method with skeins of Berlin worsted, were at this trial still greater, from different causes. In Sweden I had previously only found one case, and this (found in the summer of 1879) became unhappily useless through an accident.

Since I succeeded in finding a practical way of lessening those difficulties, I have, within a comparatively short time (June to October, 1880), been fortunate enough to examine two such cases, one of one-sided violet-blindness, and the other (for which I have to thank Professor Hippel, of Giessen), a case of one-sided red-blindness.

Experience will probably show that such cases are not so rare as we have hitherto thought, and we have every reason to hope for a speedy and perfect solution of the problem in this way; but the results of these two cases have been so remarkable that I will here give a short description of them.

The plan, principle, and result were as follows:—

First the diagnosis of both eyes was carefully made. In both cases there was found a perfect, typical, partial colour-blindness on one of the eyes (the violet-blind on the left, and the red-blind on the right eye); the other eye had a weak colour-sense, but still so nearly normal that the principal colours were ascertained with perfect ease. A slight hesitation was only shown in distinguishing the lightest and darkest shades of those colours. Both the cases were thus perfectly fit for the purpose.

The principle of the trial was exactly this:—To let the normal eye control the perception of the abnormal one, and bring the result into a form that was perfectly plain to other normal-sighted persons.

A one-sided colour-blind person has through his normal eye a per-

fectly clear conception of the normal-eyed people's different colours, and can tell his conception, by the aid of his other eye, for other normal-sighted persons. His definitions are thus—in opposition to persons colour-blind on both eyes—perfectly reliable.

As words are always less reliable than actions (*vide* Frithiof Holmgren: "De la Cécité des Couleurs dans ses Rapports avec les Chemins de Fer et la Marine," p. 116), and a description in this case is always inferior to a shown colour, I have in every instance let the person in question point out an objective colour with his normal eye for every one of his conceptions with his abnormal one.

Indirectly we find in this way which qualities of perception are wanting in the abnormal eye in comparison with the normal one. The same result is directly arrived at by letting the colour-blind eye control the subjective perception of the normal.

It is my intention to explain the application of this principle in my more detailed work on this subject, as well as the particulars of the result.

I will now only give the chief points of this result, which perhaps is done in the shortest and easiest way by giving the details of the colour-blind person's subjective spectrum. If we take the objective solar spectrum for a starting point, and choose our own definition of the different colours from the subjective spectrum of the normal eye, we come to the following results:—

As we have long supposed, for good reasons, a colour-blind person sees only two colours in the spectrum. These are his two subjective principal colours.

The principal colours in the spectrum of a *violet-blind* person are, as to their fundamental tone, *red* and *green*. Towards the red end his spectrum has quite the same extension as that of a normal-eyed person, and is thus, in comparison with the latter, "unshortened." Reckoned from the red end, his first fundamental colour stretches over that part of the spectrum which is generally seen as *red*, *orange*, and *yellow*. First in the yellowish-green (a little on the other side of Fraunhofer's line D) he sees a narrow, uncoloured ("paper-white") belt, from which his other colour, *green*, commences, and is continued with at first more and more saturated, and afterwards darker and darker shades, over the place where we see *green*, *greenish-blue*, *cyan-blue*, and *indigo* to the commencement of the violet, where his spectrum absolutely ends with a sharp limit (about Fraunhofer's line G). His spectrum is thus at this end considerably "shortened." The fact that violet-blind persons confuse the pigment colours (such as *green* and blue, *purple* and red, orange and yellow, *violet* and yellowish-green and grey) is thus explained of itself.

All this is in the main consistent with the Young-Helmholtz theory. Respecting the tone of the violet-blind person's subjective funda-

mental colours, it may be said that his red is not quite identical with the common spectral red of the normal-eyed (something like cinnabar), but rather a clearer red, having a shade of carmine, about the same as the red towards the end of the subjective spectrum of the normal-eyed. His other fundamental colour, *green*, is also a clear green that for the normal eye has a shade of blue in it.

The two principal colours in the spectrum for the *red-blind* are as to their fundamental tone *yellow* and *blue*. This yellow commences a little later, reckoned from the end, than the red of the normal-eyed (about Fraunhofer's line C), and stretches over the rest of the *red*, *orange*, *yellow*, *yellowish-green*, and ends in the *blue-green* (between Fraunhofer's lines *b* and F, nearer to the latter), where a narrow, neutral, colourless belt forms the limit against the other principal colour, *blue*, which stretches through the remaining part of the spectrum, corresponding with our *cyan-blue*, *indigo*, and *violet*. At this end there is no "shortening." The red-blind person's confusing of pigment-colours (*green* and yellow, orange and red, *purple* and blue and violet *red* and blue-green and grey) is equally well explained by this.

All this, as we see, is objectively taken in perfect accordance with the Young-Helmholtz theory. Regarded from a subjective point of view, we should perhaps have expected green instead of yellow as one of the fundamental colours. But that yellow, and not green, is that colour (as I have already for some time supposed, *vide* Upsala "Läkareförenings Förhandlingar," vol. vii, 1871, p. 119, and "Centralblatt f. d. med. Wissenschaften," 1872, p. 826) does not shake the basis of that theory, as is shown by Fick ("Zur Theorie der Farbenblindheit," 1873) and by myself ("Om Färgblindhetens Theori," 1878). Besides, the tone of the red-blind person's first fundamental colour is not perfectly golden-yellow, but seems for the normal eye to have a shade of greenish-yellow, perhaps best defined as citron-yellow in the lighter, and as olive-green in the darker, shades. His other fundamental colour does not seem to be purely cyan-blue or indigo, but is rather a blue with a perceptible shade of violet. It might be called indigo-violet.

Perfect clearness in the theory will not perhaps be gained until we shall have had opportunity of studying more cases of different kinds and degrees, and especially a case of typical perfect *green-blindness*.

Still the path is opened, and a more definite starting-point has been found for the treatment of the theoretical problem of colour-blindness, of which it is my intention to speak more explicitly in my larger work on the same subject.

III. "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." By JOHN TYNDALL, F.R.S. Received January 3, 1881.

The Royal Society has already done me the honour of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question: the latest of whom,* while justly recognising the extreme difficulty of the subject, and while verifying, so far as their experiments reach, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapours.

None of these investigators appear to me to have realised the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognise the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed, some time ago, the threads of the inquiry. The results shall, in due time, be communicated to the Royal Society; but, meanwhile, I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvanometer, it occurred to me several weeks ago that the results thus obtained might be checked by a more direct and simple form of experiment. Placing the gases and vapours in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapours ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea. But, at this point, and before my proposed gas-thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

* MM. Lecher and Pernter, "Philosophical Magazine," January, 1881. "Sitzb der K. Akad. der Wissensch. in Wien," July, 1880.

From the first, I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapours really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds, while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made, with a view of testing this idea, was executed in the presence of Mr. Graham Bell;* and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapours employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo-machine, worked by a gas engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about 7 inches distant from the lens. A circle of sheet zinc provided first with radial slits and afterwards with teeth and interspaces, cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits produced the desired intermittence,† while a flask containing the gas or vapour to be examined received the shocks of the beam immediately behind the rotating disk. From the flask a tube of india-rubber, ending in a tapering one of ivory or box wood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. Graham Bell, the arrangement here described is rude; it is, however, very effective.

* On the 29th November: see "Journal of the Society of Telegraph Engineers," December 8, 1880.

† When the disk rotates the individual slits disappear, forming a hazy zone through which objects are visible. Throwing by the clean hand, or better still by white paper, the beam back upon the disk, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colours of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colours disappear. The flashes also produce a giddiness sometimes intense enough to cause me to grasp the table to keep myself erect.

With this arrangement the number of sounding gases and vapours was rapidly increased. But I was soon made aware that the glass lenses withdrew from the beam its most effectual rays. The silvered mirrors employed in my previous researches were therefore invoked; and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results which I have now the honour to submit to the Society were obtained.

Sulphuric ether, formic ether, and acetic ether being placed in bulbous flasks,* their vapours were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disk, so that the intermittent beam passed through the vapour, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapours which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapours. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapours of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

It is the vapour, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapour-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rocksalt cell filled entirely with a volatile liquid, and subjected to the intermittent beam, produced no sound. This cell was circular and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled, the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disk and allowing its slits to act like the perforations of a syren.

* I have employed flasks measuring from 8 inches to $\frac{3}{4}$ths of an inch in diameter. The smallest flask, which had a stem with a bore of about $\frac{1}{8}$th of an inch in diameter, yielded better effects than the largest. Flasks from 2 to 3 inches in diameter yield good results. Ordinary test-tubes also answer well.

Thus, as regards vapours, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit-lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide, the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas, it gave birth to a musical note which, when the beam was in good condition, and the bulb well chosen, seemed as loud as that of an ordinary organ pipe.* We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand in hand.

A soap bubble blown with nitrous oxide, or olefiant gas, and exposed to the intermittent beam, produced no sound, no matter how its size might be varied. The pulses obviously expended themselves upon the flexible envelope, which transferred them to the air outside.

But a film thus impressionable to impulses on its interior surface, must prove at least equally sensible to sonorous waves impinging on it from without. Hence, I inferred, the eminent suitability of soap bubbles for sound lenses. Placing a "sensitive flame" some feet distant from a small sounding reed, the pressure was so arranged that the flame burnt tranquilly. A bubble of nitrous oxide (sp. gr. 1·527) was then blown, and placed in front of the reed. The flame immediately fell and roared, and continued agitated as long as the lens remained in position. A pendulous motion could be imparted to the bubble, so as to cause it to pass to and fro in front of the reed. The flame responded, by alternately roaring and becoming tranquil, to every swing of the bubble. Nitrous oxide is far better for this experiment than carbonic acid, which speedily ruins its envelope.

The pressure was altered so as to throw the flame, when the reed sounded, into violent agitation. A bubble blown with hydrogen (sp. gr. 0·069) being placed in front of the reed, the flame was immediately stilled. The ear answers instead of the flame.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the

* With conjugate mirrors the sounds with olefiant gas are readily obtained at a distance of twenty yards from the lamp. I hope to be able to make a candle flame effective in these experiments.

intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sounds may be always intensified. The ordinary temperature, however, suffices in all the cases thus far referred to.

In this relation the vapour of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling-point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapour.

Small wreaths of haze, produced by the partial condensation of the vapour in the upper and cooler air of the flask, were, however, visible in this experiment; and it was necessary to prove that this haze was not the cause of the sound. The flask was, therefore, heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapour, however, the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but, notwithstanding the tenuity of the vapour, the sound extracted from it was not only distinct but loud.

Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp until all visible humidity had been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapours, and it cannot be otherwise than satisfactory to me to find that particular vapour, whose alleged deportment towards radiant heat has been so strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapour, we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An acci-

dental observation will illustrate this point. A flask was filled with coal-gas and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open upon a table, and permitted to remain there for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.*

Transferring a small flask or a test-tube from a cold place to the intermittent beam, it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporisation by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapours absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapour, would detach the effective rays, and thus quench the sounds. The experiment was made, and the conclusion verified. A layer of water, formic ether, sulphuric ether, or acetic ether, $\frac{1}{8}$th of an inch in thickness, rendered the transmitted beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapours—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound, the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds, the displacement was prompt and forcible.

(Received January 10, 1881.)

Further Experiments.

Since the handing in of the foregoing note, on the 3rd of January, the experiments have been pushed forward; augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

* The method here described is, I doubt not, applicable to the detection of extremely small quantities of fire-damp in mines.

All the results described in my first note have been obtained in a very energetic form with a battery of sixty Grove's cells.

On the 4th of January I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector the stronger vapours rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapours.

Abandoning the mirror and bringing the candle close to the rotating disk, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disk, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disk, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments, an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three, the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapour had led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks and permitted to remain in the laboratory from the 23rd of December to the 4th of January. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due, not to dry air, but to traces of aqueous vapour.

An ordinary bottle, containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent

beam, emitted a faint, but distinct, musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some days, was placed in the beam: the dry air above the liquid proved absolutely silent.

On the evening of January the 7th Professor Dewar handed me four flasks treated in the following manner. Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium; while the fourth contained a small quantity of phosphoric anhydride. They were closed with well fitting india-rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles, and sometimes destroys the sound.

A bulb, less than a cubic inch in volume, and containing a little water, lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit lamp, the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb* is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapour. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapour.

The action of aqueous vapour being thus revealed, the necessity of thoroughly drying the flasks, when testing other substances, becomes obvious. The following plan has been found effective. Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400° C. While the flask is still hot, a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapour by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately to the intermittent beam, the attention of the ear, if I may use the

* In such bulbs even bisulphide of carbon vapour may be so nursed as to produce sounds of considerable strength.

term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868, I determined the absorptions of a great number of liquids whose vapours I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtingly twelve years ago that the vapour of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed, and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved in 1861 to be absorbers of radiant heat. I limit myself here to the vapours of pachouli and cassia, the former exercising a measured absorption of 30, and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of pachouli.

Many years ago I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask, and permitted to displace the air within it. Exposed to the intermittent beam, its sound was similar in power to that of marsh-gas.

The specific gravity of marsh-gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval, the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapour rapidly diffused itself in the air above the liquid. Placed in the intermittent beam, a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapour. But my former experiments on this vapour were conducted with obscure heat; whereas, in the present instance, I had to deal with the radiation from incandescent lime, whose heat is, in part, luminous. Now the colour of the bromine vapour proves it to be an

energetic absorber of the luminous rays; and to them, when suddenly converted into thermometric heat in the body of the vapour, I thought the sounds might be due.

Between the flask containing the bromine and the rotating disk, I therefore placed an empty glass cell: the sounds continued. I then filled the cell with transparent bisulphide of carbon: the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission: the sounds were immediately stilled.

Iodine vaporised by heat in a small flask, yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might indeed have been foreseen, that the rays transmitted by the iodine as a liquid, would also be transmitted by its vapour, and thus fail to be converted into sound.*

To complete the argument:—While the flask containing the bromine vapour was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disk. There was no sensible abatement of the sounds with either bromine or iodine vapour.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disk. In the next experiment they were rendered parallel by the mirror, and afterwards rendered convergent by a lens of ice. At the focus of the ice-lens the sounds were extracted from both bromine and iodine vapour. Sounds were also produced after the beam had been sent through the alum solution and the ice-lens conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapours at a distance of 100 feet from the source of rays.

Several vapours other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapours of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the vapours of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

(Received January 14.)

Note.—With some of the strongest sounds, which were audible when the ear-tube was entirely withdrawn from the ear, I tried to obtain the

* I intentionally use this phraseology.

agitation of a soap-film. A glass tube, blown into a shape somewhat resembling a tobacco-pipe, had its mouth closed by such a film, while its open stem was connected with a sounding flask. I did not succeed in producing any visible agitation. When the film was uniformly illuminated, or when it had become thin enough to produce iridescent colours, on holding a high-pitched tuning-fork near the open end of the stem, the whole surface of the film was immediately covered with concentric rings, having the centre of the film for their centre. This belongs to the class of effects so vividly described by Mr. Sedley Taylor. A fork of the pitch of the sounding gas produced no visible effect upon the film.

January 20, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Gravimeters, with special reference to the Torsion-Gravimeter designed by the late J. Allan Broun, F.R.S." By Major J. HERSCHEL, R.E., F.R.S., Deputy-Superintendent of the Great Trigonometrical Survey of India. Received October 31, 1880.

(Abstract.)

In the course of 1878–9 some correspondence passed between Major-General Walker, the Surveyor-General of India, and the late Mr. Broun, on the subject of an instrument designed by the latter, the fate of which General Walker was desirous of learning. Interrupted by the death of Mr. Broun, General Walker requested the Director-General of Stores for India to inquire if the instrument could be procured for the Indian Government. It had meanwhile become the property of the Royal Society. In order to comply with the Surveyor-General's intention, the loan of the instrument was then obtained from this Society, and it was placed in the hands of the author of this paper to examine and report upon.

The paper consists mainly of a narrative account of the examination, and the conclusions to which he has been led regarding instruments of this class.

A very similar, if not identical, design having been published in the "Comptes Rendus" of the Paris Academy, by the late M. Babinet, the author contributes to the history of the subject some letters written by Babinet anterior to the said publication, with the object of showing the concurrent identity and independence of the two designs.

The principle on which the bathometer of Dr. Siemens appears to be constructed is then considered. Although intended to measure sea-depths, and on that account called a bathometer, it is none the less a gravimeter in principle, and its construction, when properly understood, reminds one forcibly of the typical form as originally proposed, by way of illustration, by Sir John Herschel in the "Outlines of Astronomy." The indications of the bathometer, it should be said, are only to be interpreted empirically.

The principle on which the gravimeters of Broun and Babinet are designed is thus emphasized by the latter:—"Ce qui distingue surtout ma méthode, c'est le fractionnement de la pesanteur, de manière a équilibrer l'éffet d'un poids considérable, par la torsion d'un fil metallique de force moyenne, et restant dans les limites de l'élasticité parfaite."

It is doubtful whether Babinet's conception ever took practical form as an actually constructed instrument; at any rate, no published record of its performance exists. Broun's design, on the other hand, after many vicissitudes, took the shape of the instrument now under consideration. It was made for him by Müller of Stuttgart, who appears to have taken extraordinary pains to give effect to the intentions of the designer.

Mr. Broun exhibited his gravimeter in the Loan Collection of Scientific Instruments at South Kensington, in 1876, where it was placed alongside of Dr. Siemens' bathometer. A description of it appeared in the catalogue, corrected by himself, but it is faulty and inadequate, owing to the declining health of the owner having prevented his acquiring a practical acquaintance with his instrument; nor was he able to do so up to the time of his death. It was, therefore, with the very scantiest preparation that the present examination was undertaken.

The principle of Broun's gravimeter is easily understood, but the manipulation of the instrument is so troublesome that it is more than doubtful whether in its present form results of any practical value could be obtained with it. The various grounds for this opinion are explained in the course of the report. As a fact, no observations, such as were naturally desired to test its sensitiveness, could by any means be obtained, owing to a radical defect of construction, which the maker has unfortunately failed in overcoming, although special attention has been bestowed upon the point. To explain this, some description of the instrument is unavoidable.

If a heavy bar be suspended by two parallel inelastic lines, the force which is exerted when the position of rest has been quitted varies, in all positions, as the force of gravity at the time: if the angle of disturbance and all other attendant circumstances be strictly the same, the proportion between the vertical and horizontal forces is constant; and if the suspending lines are long, as compared with their distance apart, this constant is a small fraction. This is "le fractionnement de la pesanteur" of which Babinet speaks. We are to regard the horizontal force as representing gravity and varying with it, at different times or places. Now, if the suspension had been by a single instead of a double line, and that elastic, the horizontal force exerted in consequence of disturbance would have been independent of the weight of the bar, and therefore also of variation of gravity. It would be a constant force, as far as gravity is concerned.

In the torsion gravimeter these two kinds of suspension are combined—the constant being adjusted so as to balance exactly the inconstant force, by the addition or subtraction of a minute subsidiary weight to the primary weight carried by the double lines.

It would be difficult to explain briefly the mechanism by which the condition of equilibrium is ascertained to be perfectly secured: enough has been said to render the source of failure intelligible.

Success must evidently depend on constancy in the force exerted by the single wire; and this on the fixity of attachment of the ends of the same. It appeared by repeated trials and experiments of different kinds that this attachment was faulty: further experiments, having for their object to remedy the defect, proved unavailing; and at last the attempt—and with it the intended trial of the instrument as a gravimeter—was abandoned, as under the circumstances hopeless.

Unsatisfactory as this result undoubtedly is—especially for the credit of so developed an instrument—the inquiry has not been entirely barren of useful result. The theory of the machine had not been fully considered, and at least one important point would seem to have escaped recognition altogether. There is reason to think that had it been seen that the double suspending lines must practically perform a function precisely similar to that for which the single wire is provided, the latter would have been dispensed with. It is true that this implies not only other modifications of design but also the further recognition of the part actually played by the torsion of the double wires, of which no sign appears in anything that has yet been published about it. But it is scarcely doing violence to the ingenuity which developed a complicated machine to suppose that it would have seized upon and utilised a mode of simplification had attention been directed to it.

The simplification to which allusion is here made, has been brought to the notice of the Society in a previous paper.

In some other respects also, the examination may be said to have borne fruit: for occasion was taken to determine the weights and dimensions of parts, and to show the degree of accuracy which might be looked for in such an instrument, supposing it capable of fulfilling its intended purpose. Some defects of construction also of a general character have been pointed out which may serve as hints to future constructors.

It should be said in conclusion, that so much of the present paper as deals with Broun's gravimeter in particular, was prepared and submitted as a report in the first instance, to the India Office, and that it was communicated to the Society for publication, by permission of the Secretary of State for India in Council. What may be called the historical part was subsequently added, by way of introduction, to make it more worthy of that honour.

II. "Experimental Researches into Electric Distribution as manifested by that of the Radicles of Electrolytes." By ALFRED TRIBE, F.I.C., Lecturer on Chemistry in Dulwich College. Communicated by Dr. GLADSTONE, F.R.S. Received December 15, 1880.

(Abstract.)

The paper is divided into three principal parts; the distribution of the ions of copper sulphate on silver plates; analogies between the distribution of ions and that of static electricity; and analysis of electrolytic fields.

When a solution of copper sulphate is electrolysed with silver electrodes, the negative electrode becomes covered with a deposit of copper, the positive with a coating of silver peroxide. The author has shown that if a metallic rod or plate is placed lengthwise between the electrodes in a liquid undergoing electrolysis, the plate does work identical in kind with that being done by both electrodes. It follows that a silver plate in copper sulphate will be found coated on one part with copper, and on the other with silver peroxide. The superficial distributions of these ions were examined. A rectangular electrolytic cell, a solution of copper sulphate, and copper electrodes were employed. The electrodes were of the same breadth and depth as the cell. In the centre of the liquid, lengthwise and perpendicular to the electrodes, a plate of thin sheet silver was fixed.

In all cases the distributions of the two ions on the plates or even on bodies of various other shapes, were invariably unsymmetrical, the negative ion generally spreading further than the positive. The dis-

tributions are the same when the experiment is begun with a plate covered with either ion, or when the action is continued for a longer time. But a change in the strength of solution, or in temperature, or in strength of current alters the distributions. Speaking generally, there is an increase in the range of the positive ion with increase of temperature, increase of current strength, and decrease in strength of solution; while the range of the negative ion decreases with increase of temperature, increases to a certain extent with increase of current, and varies irregularly in the case of change in the strength of solution.

The mutual influence of plates, end to end and side by side, the distribution on open tubes, and on plates encircled by them or copper gauze, show general analogies with what is known of the action of static electricity.

As long as the plates are perpendicular to the electrodes, the distributions of one ion on the two *sides* is the same, but when the plate forms an angle with a line joining the electrodes, this no longer holds true, and the boundary lines of the same ion become markedly concave on one side and convex on the other.

With plates parallel to the electrodes, hardly any action took place until the shape of the plate was altered, and it is shown that the relation between the length and breadth of a plate of given area has a very material effect in determining the accumulation and distribution. And it was also found that the surface of a plate in this position for a measurable distance from its edges, and still more from its angular projections, shows no signs of electrification even when the body of the plate does so to a sensible degree. These results afford a new basis for investigating the field of electrolytic action both as regards physical differences and the relative direction in which the action is transmitted.

With electrodes of the same breadth and depth as the cell, the same distributions were obtained in different parts of the field of action, but when the electrodes were not of the same breadth and depth as the cell, differences were obtained in absolutely corresponding parts of the field. These differences increased as the electrodes became narrower. It is evident that the smaller electrodes give a non-homogeneous field.

The distributions are altered by the interposition of an ebonite screen between an electrode and an analysing plate even in a field otherwise homogeneous, and some instances of this are given. Analogies are found between the effect of these screens and that produced by diminishing the size of the electrodes.

The distributions are divisible into two classes—parallel and non-parallel. The first class includes all cases which show a similarity in the distribution on the two sides of the analysing plate; the second

class, all cases which show a difference in the configuration of the boundaries on the two sides of the plate. All the distributions in the experiments with different temperatures, strengths of solution and of current, belong to the first class; also those distributions obtained in the non-homogeneous fields, and with ebonite screens.

On analysing a non-homogeneous field of larger dimensions, parallel distributions were obtained all along a line running between the electrodes, as well as along a central line at right angles to this. But for some distance from the electrodes at the ends, the plates showed non-parallel distributions.

The paper concludes by drawing attention to the simplicity of the method, and the permanent form of the self-recorded results, and indicates the direction in which it is desirable to extend the research.

III. "On the Tidal Friction of a Planet attended by several Satellites, and on the Evolution of the Solar System." By G. H. Darwin, F.R.S. Received December 27, 1880.

(Abstract.)

The first part of the paper contains the investigation of the changes produced by tidal friction in a system consisting of a planet with any number of satellites revolving round it in circular orbits. The planet's equator and the satellites' orbits are all supposed to be in one plane. The planet is formed of homogeneous viscous fluid, but a large part of the results, due to the particular sort of tidal friction which arises in this special case, would be equally true under a more general hypothesis as to the nature of the planet. The mutual perturbations of the satellites are neglected, so that only the rotation of the planet and the distances of the satellites have to be considered.

It is then proved that if E be the whole energy, both kinetic and potential, of the system, and if ξ be a function of the distance of any one of the satellites from the planet (which function, when the mass of the satellite is small compared with that of the planet, is the $\frac{1}{2}$ power of the distance), the equation expressive of the rate of change of ξ is

$$\frac{d\xi}{dt} = -A\frac{\partial E}{\partial \xi}$$

where t is the time, A a certain constant, and ∂ expresses partial differentiation.

A similar equation applies to each satellite, and the whole of the equations form a system of simultaneous differential equations, which

have to be solved in order to trace the changes in the system of satellites.

Expressions are also found for the rotation of the planet, and for the energy E, in terms of the resultant moment of momentum of the system and of the ξ's.

It is then shown how these equations may be solved by series, proceeding by powers of the time. As, however, the series are not rapidly convergent, they are not appropriate for tracing extensive changes of configuration.

The case where there are only two satellites is then considered in detail, and it is shown that, if a surface be constructed, the points on which have E and the two ξ's as their three rectangular co-ordinates (E being drawn vertically upwards and the ξ's being horizontal), then the solution of the problem is expressed by the statement that the point, representing on the surface the configuration of the system, travels down the steepest path.

The contour-lines on this "surface of energy" are illustrated by figures, and the graphical solution found therefrom is interpreted and discussed.

The second part of the paper contains a discussion of the part played by tidal friction in the evolution of the solar system.

It is proved that the rate of expansion of the planetary orbits which arises from the friction of the tides raised by the planets in the sun must be exceedingly small compared with that which arises from the friction of the tides raised by the sun in the planets. Thus the investigation in the first part of the paper, where the satellites are treated as particles, is not applicable to the solar system.

Although the problem of finding the changes in a system, formed by a rigid or perfectly fluid sun attended by tidally disturbed planets, is easy of solution, yet it seemed inexpedient to attempt a numerical solution which should be applicable to the solar system.

It appeared, however, likely that a knowledge of certain numerical values would throw light on the question. Accordingly the moments of momentum of the orbital motion of the planets round the sun, of the sun's rotation round his axis, of the orbital motion of the satellites round their planets, and of the rotation of the planets about their axes are evaluated with such degree of accuracy as the data permit.

From a comparison between the orbital momenta of the planets and their rotational momenta, it is concluded that tidal friction can scarcely sensibly have enlarged the planetary orbits since the planets had a separate existence.

By parallel reasoning (although the argument has much less force) it also seemed improbable that the orbits of the satellites of Mars, Jupiter, and Saturn have undergone very large extensions since the satellites had separate existences, and it seemed nearly certain that

they cannot be traced back to an origin almost in contact with the present surfaces of their planets, as was shown in previous papers to be probably the case with the moon and earth.

The numerical values spoken of above exhibit a very striking difference between the condition of the earth and moon and that of these other planets, and it may therefore be admitted that their modes of evolution have also differed considerably.

The part played by tidal friction in the evolution of planetary masses is then discussed.

A numerical comparison is made of the relative efficiency of solar tidal friction in reducing the rotational momentum and the rotation of the several planets. It is found that the efficiency as regards the rotation is nearly the same for Mars and for the earth, notwithstanding the greater distance of the former from the earth. This point is important with reference to the rapid revolution of the inner satellite of Mars, and confirms the explanation of this fact, which has been offered in a previous paper.

The numbers expressive of the relative efficiency of solar tidal friction are of course very much smaller for the more remote planets than for the nearer ones, but they must not be supposed to represent the total amount of rotation destroyed by solar tidal friction, because the exterior planets must be presumed to have existed much longer than the interior ones. Nevertheless the disproportion between the numbers is so great that it must be held that the influence of solar tidal friction on Jupiter and Saturn has been considerably less than on the nearer planets.

The manner in which tidal friction and the contraction of a planetary mass would work together is then considered, and it is found to be probable that tidal friction was a more important cause of change when the masses were less condensed than it is at present; thus the present rate of action of solar tidal friction is not to be taken as a measure of what has existed in all past time.

This discussion leads the author to assign a cause for the observed distribution of satellites in the solar system. For if, as the nebular hypothesis supposes, satellites are formed when instability is produced by the acceleration of rotation accompanying contraction, then the epochs of instability would recur more rarely if tidal friction were operative than without it; and if tidal friction were sufficiently powerful, an epoch of instability would never occur.

The efficiency of solar tidal friction diminishes as we recede from the sun, and therefore planets near the sun should have no satellites, and the number of satellites should increase for the remoter planets. This is the observed condition of the solar system.

This theoretical view is also shown to explain how the earth and moon came to differ from the other planets in such a manner as to

permit tidal friction to be the principal feature in their evolution, whilst its effects are less striking in the other planets.

Amongst other points discussed are the comparative speeds of rotation of the several planets, and the probable effects of the genesis of a satellite on the course of change afterwards followed by the planet.

The paper ends with a review of the solar system, in which it is shown that the tidal hypothesis is a means of co-ordinating many apparently disconnected phenomena, besides giving a history of the earth and moon since the origin of the latter.

These investigations afford no grounds for the rejection of the nebular hypothesis, but while they present evidence in favour of the main outlines of that theory, they introduce modifications of considerable importance. Tidal friction is a cause of change of which Laplace's theory took no account, and although the activity of that cause is to be regarded as mainly belonging to a later period than the events described in the nebular hypothesis, yet its influence has been of great, and in one instance of even paramount importance, in determining the present condition of the planets and of their satellites.

IV. "On the Female Organs and Placentation of the Racoon (*Procyon lotor*)." By M. WATSON, M.D., Professor of Anatomy, Owens College, Manchester. Communicated by Professor HUXLEY, Sec. R.S. Received December 30, 1880.

(Abstract.)

The paper contains an anatomical description of the female organs and placenta of the racoon. The specimen examined contained but a single fœtus, which was lodged in the right horn of the uterus. The uterine mucous membrane of the unimpregnated horn was richly supplied with glands, which presented the usual structure. In the non-placental area of the gravid horn these glands, although present, were evidently undergoing degeneration and were with difficulty recognised, and then only in a fragmentary condition; whilst in the placental area of the uterine mucous membrane these glands had entirely disappeared.

The author's observations upon the placenta of *Procyon lotor* show that in respect of this organ:—(1.) *Procyon agrees* with all other carnivora, in which that organ has hitherto been examined, in the possession of a *zonary* or *annular* placenta.

(2.) That *Procyon agrees* with all of these in the mode of interlocking of the fœtal and maternal portions of the placenta and in the consequent deciduate character of that organ.

(3.) That *Procyon agrees* with the members of the *plantigrade* sec-

tion, at the same time that it differs from those composing both the *digitigrade* and *pinniped* sections of the carnivora, inasmuch as at one spot the placenta presents a deficiency or gap, at which spot the placental structure is imperfect.

(4.) That *Procyon agrees* with *Canis*, at the same time that it differs from *Felis*, in the absence of a continuous layer of *decidua serotina* from the uterine surface of the detached placenta.

(5.) That *Procyon differs* from every other carnivore, the placenta of which has been minutely examined, in the possession of placental vessels possessed of a structure hitherto only met with in the placenta of *Cholœpus Hoffmanni* among the Edentates.

(6.) That *Procyon differs* from every other carnivore, the fœtus of which has been hitherto examined, in the non-possession of an umbilical vesicle.

(7.) That *Procyon differs* from all carnivora of which the young have been hitherto examined, inasmuch as the fœtus is provided with a supernumerary cuticle or *epitridermia*, a structure which has only been met with in the young of certain members of other mammalian groups.

From an examination of the various facts detailed in the paper, the author arrives at the conclusion that a consideration of the modifications in form and structure of the placenta *alone* is insufficient as affording a basis upon which to found a natural classification of the mammalia.

V. "Further Note on the Minute Anatomy of the Thymus" By Herbert Watney, M.A., M.D. Cantab. Communicated by E. A. Schäfer, F.R.S. Received January 3, 1881.

In the involution of the thymus, connective tissue is formed in the medullary portion of the follicle; and to some extent in the cortical. Besides this, the connective tissue outside the follicles increases, and in this connective tissue fat is formed; by these means the gland is finally replaced by fat and fibrous threads.

The cortical part attains its greatest size at the time of the greatest activity of the gland, it is not differentiated from the rest of the follicle at a very early period and disappears somewhat sooner than the medullary portion.

The lymphatics of the thymus of the calf are small. In the neck they run to neighbouring lymphatic glands, the efferent vessel of which runs to two other lymphatic glands lying on the trachea at the root of the neck. There is no connexion with the large lymphatics of the neck. In the thoracic portion of the thymus the lymphatics can also be traced to lymphatic glands, one of which is generally situated on the aorta.

The reticulum of the cortical part of a follicle is of two kinds; consisting (1) of a reticulum, composed of coarse threads and of delicate cells, with long branching processes—these cells and threads are attached to the blood-vessels. In the meshes of this reticulum two different kinds of small bodies are seen nearly equal in size; the one (*a*) are very highly refractive and angular, and have short threads attached to their angles, the other (*b*) circular and much less refractive—*a* and *b* are acted upon differently by staining solutions. The highly refractive bodies form the nodal points (2) of a delicate reticulum which encloses the circular less highly refractive cells.

Traces of this fine reticulum can be seen in the medullary portion.

The granular cells mentioned in a preceding note ("Proc. Roy. Soc.," vol. 27, p. 369) take their origin in the connective tissue cells which constitute the network of the medullary portion. These granular cells not only help to form the concentric corpuscles, but are actively concerned in the formation of fibrous tissue; their fibrillated processes are sometimes found to be attached to newly formed connective tissue.

The granular cells are identical with some forms of giant cells—they are not the plasma cells of Waldeyer, although plasma cells are present in the thymus, as has been described by Ehrlich.

January 27, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "The Refraction Equivalents of Carbon, Hydrogen, Oxygen, and Nitrogen in Organic Compounds." By J. H. GLADSTONE, Ph.D., F.R.S. Received January 4, 1881.

Since the communication which I had the honour to read before this Society in 1869, "On the Refraction Equivalents of the Elements," very little has been done on the subject. My own contributions have been almost confined to two communications in the "Journal of the Chemical Society," in 1870; the one a lecture on the subject in general, the other a paper on the "Refraction Equiva-

lents of the Aromatic Hydrocarbons and their Derivatives;" together with a discourse at the Royal Institution in March, 1877, on "The Influence of Chemical Constitution on the Refraction of Light." In the meantime, observations on many substances have gradually accumulated in my note-book.

Of late, however, the importance of the subject in regard to theories of chemical structure has been recognised by Dr. Thorpe and other chemists in this country, and attention has been recalled to it in Germany by the papers of Brühl, who, following closely in the footsteps of Landolt, has endeavoured to explain the results in the language of modern organic chemistry.

At this juncture it may be of service to put on record my present views in regard to the refraction equivalents of the four principal constituents of organic bodies—carbon, hydrogen, oxygen, and nitrogen. The figures in this paper are always reckoned for the line A of the solar spectrum, the refraction equivalent being the specific refraction for A multiplied into the atomic weight, or $P\frac{\mu_A-1}{d}$. In the present stage of the inquiry, though the results are deduced from many observations, I have not thought it desirable to go beyond the first place of decimals.

Carbon.—Carbon in its compounds has at least three equivalents of refraction, 5·0, 6·0 or 6·1, and about 8·8.

Whether its refraction should be one or other of these appears to depend on the way in which the atoms are combined.

When a single carbon atom has each of its four units of atomicity satisfied by some other element, it has a value not exceeding 5·0. There are some indications that the value may be slightly less than this.

When a carbon atom has one of its units of atomicity satisfied by another carbon atom and the remainder by some other element, it has the value of 5·0, the same as in diamond. This is also the case if two of its units of atomicity are satisfied by carbon atoms. The majority of organic compounds of course fall into this category.

When a carbon atom has three of its units of atomicity satisfied by other carbon atoms, its value is 6·0. The most striking instance is that of benzol, C_6H_6 (refraction equivalent 43·7), in which it is difficult to conceive that each carbon atom is not in the condition just described, and which, reckoning 1·3 for each hydrogen, gives a little less than 6·0 for each carbon. Styrol, C_8H_8 (57·8), gives a similar value.

There are other organic compounds in which only some of the atoms of carbon have the higher value. It has been especially the work of Brühl to point this out, and to show that where they occur (as in amylene or the allyl compounds) the carbon atom is in a condition similar to those in the phenyl nucleus, that condition in fact

which is generally represented in our graphic formulæ by two carbon atoms linked by double bonds.

The value assigned by Brühl in such cases is, however, 6·1. This somewhat higher figure is deduced from the aggregate value of the six carbon atoms in the nucleus of the aromatic series, which (except in benzol and its simpler substitution products) would appear to be nearer 37 than 36. If equally distributed over the six atoms this would give a value of at least 6·1 for each. The fact, however, is susceptible of another interpretation. It does not follow that in these more complicated bodies all the carbon atoms are exerting the same influence on the rays of light. The replacement of hydrogen by some monad radicle is an important change; and if that radicle be CH_3 it is evident that according to present views the carbon atom must have all four of its units of atomicity satisfied with carbon, and by analogy we should expect it to have its refraction increased. What that increased value may be, or which indeed of the two hypotheses is most in accordance with the facts, it seems to me that we have not yet sufficiently accurate data for determining.

When a carbon atom has all four of its units of atomicity satisfied by other carbon atoms, each of which has the higher value of 6·0 or 6·1, its equivalent of refraction is greatly raised. There are compounds in which the atoms of carbon actually outnumber the atoms of hydrogen or its substitute, such as naphthalene, $C_{10}H_8$ (ref. eq. 75·1), naphthol, $C_{10}H_8O$ (79·5), phenanthrene, $C_{14}H_{10}$ (108·3), and pyrene, $C_{16}H_{10}$ (126·1). That the refraction is greatly raised is evident from the fact that, if we were to reckon all the carbon atoms at 6·1, the refraction equivalent of the body would not be fully accounted for. It is evident that in pyrene only ten of the atoms of carbon can be in the same condition as they are in benzol or styrol, the other six must have all their units of atomicity satisfied by carbon alone. Now, if we allow 6·0 as the value of each of the ten carbons, and 1·3 for each of the ten hydrogens, we get 73·0, which taken from 126·1 leaves 53·1 for the remaining six atoms of carbon, or 8·8 for each. By a similar calculation the four extra atoms in phenanthrene are found to have the value of 8·8 each. Taking oxygen at 2·9, naphthol gives 9·1 for each. But the experimental data do not indicate a higher value than 8·4 for each of the extra carbon atoms in naphthalene. Provisionally I venture to assign 8·8 as the refraction equivalent of this highest carbon.

There are several other bodies, such as anthracene, anethol, furfurol, and hydride of cinnamyl, which from their abnormally high refraction appear to contain carbon in this last condition.

Hydrogen.—The general evidence with regard to hydrogen in organic compounds tends to show that it has only one refraction equivalent, that originally assigned to it by Landolt, 1·3.

Oxygen.—Brühl has been the first to point out that oxygen in organic

compounds has two values, and he comes to the conclusion that it has the value 3·35 where the oxygen is attached to a carbon atom by a double linking, but 2·76 in hydroxyl and where the oxygen is united to two other atoms.* This is deduced from experimental data: but there are other results which present difficulties. Thus the refraction of no substance is more certainly known than those of water, wood spirit, and alcohol. But the oxygen in H_2O (5·9) appears to have the higher number 3·3, notwithstanding its union to two atoms of hydrogen, while in CH_4O (13·1), C_2H_6O (20·8), as well as higher alcohols, and the diatomic ethene alcohol, $C_2H_4O_2$ (23·7), and the triatomic glycerol, $C_3H_8O_3$ (33·9), the oxygen is not 2·76, but 2·9 or 3·0, the numbers originally assigned to this element.

Nitrogen.—Nitrogen has two values, 4·1 and 5·1, or thereabouts.

The lower value, 4·1, is that originally deduced from cyanogen and metallic cyanides, and it seems to be generally confirmed by the observations on organic cyanides and nitriles. The higher value, 5·1, is deduced from all my observations on organic bases and amides, such as diethylamine (39·4), triethylamine (54·6), formamide (17·4), &c.

The determination of the value of nitrogen in nitro-substitution products presents some peculiar difficulties. The observations are not accordant. Even were the value of NO_2 obtained with certainty, it would not be easy to say how much should be attributed to the oxygen, especially when it is remembered that combination with oxygen alters very materially the refraction of the analogous elements, phosphorus and arsenic.

I hope shortly to submit to the public the data for these calculations, and in fact the whole of my recent observations on the refraction of organic compounds, together with a fuller discussion of the conclusions that may be drawn from them.

II. "On certain Definite Integrals." No. 8. By W. H. L. Russell, F.R.S. Received January 6, 1881.

I commence this paper with some general reflections on the theory of definite integrals. A definite integral may be written thus—

$$\int_a^\beta dx f(a, b, c \ldots x) = \phi(a, b, c \ldots).$$

If we expand in terms of (a) and equate the coefficients of a^n we shall have

$$\int_a^\beta dx f_1(n, b, c \ldots x) = \phi, (n, b, c \ldots).$$

* These have been calculated for line A.

And again expanding in terms of b, and equating coefficients of b^m, we shall have

$$\int_\alpha^\beta dx f_2(n, m, c \ldots x) = \phi_2(n, m, c \ldots).$$

And thus we may proceed in general until we arrive at a simple definite integral containing only one arbitrary constant and the indices $n, m, \ldots$

Conversely we may obtain a complicated definite integral in many cases from a simple one, by multiplying it by constants raised to the powers of certain quantities contained as indices in the integral, assigning successive values to those indices, and then summing the resulting series. Thus the integral (123),

$$\int_0^{\frac{\pi}{2}} d\theta \cos\theta \cdot \frac{\mu\cos\theta(\beta+\cos\tan\theta)-\lambda\sin\theta\sin\tan\theta}{(1+2\beta\cos\tan\theta+\beta^2)(\lambda^2\sin^2\theta+\mu^2\cos^2\theta)},$$

was obtained from the definite integral

$$\int_0^{\frac{\pi}{2}} \cos^{n-1}\theta d\theta \cos(c\tan\theta+(n-1)\theta)$$

by a process of double summation. These considerations show us why the method of summation is of such great importance in the evaluation of definite integrals.

I now hope to prove, as I stated in the last paper, that every function of an algebraical magnitude may be regarded as a centre from which systems of definite integrals emanate in all directions like rays from a star, in such a manner that the value of each integral is equivalent to the original function transformed by a known symbol.

Let $$f(x) = A_0 + A_1 x + A_2 x^2 + A_3 x^3 + \ldots$$

Then

$$4 \cdot 3A_4 x^2 + 5 \cdot 4 \cdot A_5 x^3 + 6 \cdot 5 \cdot A_6 x^4 + \ldots = f''(x) - 2A_2 - 2 \cdot 3 \cdot A_3 x;$$

or since $$n(n-1) = \frac{2^{n+2}}{\pi}\int_0^{\frac{\pi}{2}} d\theta \cos^n\theta\cos(n-4)\theta,$$

we shall have

$$\frac{2^6 x^2}{\pi}\int_0^{\frac{\pi}{2}} d\theta\{A_4\cos^4\theta + A_5\cos^5\theta \cdot \cos\theta \cdot 2x + A_6\cos^5\theta\cos 2\theta(2x)^2 + \ldots\}$$
$$= f''(x) - 2A_2 - 6A_3 x;$$

or putting $x=\frac{1}{2}$, $$\frac{2^3}{\pi}\int_0^{\frac{\pi}{2}} d\theta\{A_4\cos^4\theta + A_5\cos^5\theta e^{i\theta} + A_6\cos^6\theta e^{2i\theta} + \ldots\}$$

$$+\frac{2^3}{\pi}\int_0^{\frac{\pi}{2}} d\theta\{A_4\cos^4\theta + A_5\cos^5\theta e^{-i\theta} + A_6\cos^6\theta e^{-2i\theta} + .\} = f''(\tfrac{1}{2}) - 2A_2 - 3A_3.$$

Hence we have

$$\frac{2^3}{\pi}\int_0^{\frac{\pi}{2}} d\theta\{\epsilon^{-4i\theta}f(\cos\theta\epsilon^{i\theta})+\epsilon^{4i\theta}f(\cos\theta\epsilon^{-i\theta})\}$$

$$=\frac{2^4}{\pi}\int_0^{\frac{\pi}{2}} d\theta\{A_0\cos 4\theta+A_1\cos\theta\cos 3\theta+A_2\cos^2\theta\cos 2\theta+A_3\cos^3\theta\cos\theta\}$$

$$+f''\tfrac{1}{2}-2A_2-3A_3=\frac{2^4}{\pi}\left(A_2\,.\,\frac{\pi}{2^3}+A_3\,.\,\frac{3\pi}{2^4}\right)+f''\tfrac{1}{2}-2A_2-3A_3.$$

Hence we shall have:—

$$\int_0^{\frac{\pi}{2}} d\theta\{\epsilon^{-4i\theta}f(\cos\theta\epsilon^{i\theta})+\epsilon^{4i\theta}f(\cos\theta\epsilon^{-i\theta})\}=\frac{\pi}{8}f''(\tfrac{1}{2})\;.\;\;.\quad(133).$$

This formula was obtained by differentiating $f(x)$ twice, but similar formulæ may be obtained by differentiating any number of times.

By analogous processes we may obtain likewise the following integrals:—

$$\int_0^{\frac{\pi}{2}} d\theta\{\epsilon^{-4i\theta}f(\cos^{\frac{1}{2}}\theta\epsilon^{\frac{i\theta}{2}})+\epsilon^{4i\theta}f(\cos^{\frac{1}{2}}\theta\epsilon^{-\frac{i\theta}{2}})\}=\frac{\pi}{8}\left\{\frac{1}{2}f''\frac{1}{\sqrt{2}}-\frac{1}{\sqrt{2}}f'\frac{1}{\sqrt{2}}\right\}$$
$$.\;\;.\;\;.\quad(134).$$

This integral requires the evaluation of

$$\int_0^{\frac{\pi}{2}}\cos^n\theta\cos\beta\theta d\theta$$

when β is greater than n, and consequently the usual formula does not apply. We may, however, proceed thus; since

$$\int d\theta\cos^n\theta\cos\beta\theta=\tfrac{3}{2}\int d\theta\cos^{n+1}\theta\cos(\beta-1)\theta-\tfrac{1}{2}\int d\theta\cos^{n+1}\theta\cos(\beta-3)\theta$$
$$-\int d\theta\cos^n\theta\cos(\beta-2)\theta+\int d\theta\cos^{n+2}\theta\cos(\beta-2)\theta\;\;.\;\;.\;\;.\;\;.\quad(135),$$

we are able by successive reductions to reduce the required integral to known forms.

$$\int_0^{\frac{\pi}{2}} d\theta\log_\epsilon\cos\theta\cos^2\theta\{\epsilon^{4i\left(\frac{\pi}{2}+\theta\right)}f\epsilon^{2i\left(\frac{\pi}{2}+\theta\right)}+\epsilon^{-4i\left(\frac{\pi}{2}+\theta\right)}f\epsilon^{-2i\left(\frac{\pi}{2}+\theta\right)}\}$$
$$=\frac{\pi}{4}\phi(1)\text{ where }\phi(x)=\iiiint f(x)dx^3\;\;.\;\;.\;\;.\;\;.\quad(136).$$

$$\int_0^{\pi} d\theta\,\Theta\sin\frac{\theta}{2}(\epsilon^{\frac{3}{2}i}f\epsilon^{\theta i}-\epsilon^{-\frac{3\theta i}{2}}f\epsilon^{-\theta i})=i\pi\phi(1),\text{ where }\phi(x)=\iiint f(x)dx^3,$$

and Θ is the quantity defined in the fifth paper of this series . (137).

$$\int_0^{\frac{\pi}{2}} d\theta \cos^2\theta \{ f\epsilon^{\cos^2\theta\epsilon^{2i\theta}} + f\epsilon^{\cos^2\theta\epsilon^{-2i\theta}} \} = \frac{\pi}{4}\left\{ 2f\epsilon^{\frac{1}{2}} + \frac{3\epsilon^{\frac{1}{2}}}{8} f'\epsilon^{\frac{1}{2}} \right\} \quad \ldots \quad (138).$$

$$\int_0^{\frac{\pi}{2}} d\theta \cos^2\theta \{ f\epsilon^{\cos\theta\epsilon^{2i\theta}} + f\epsilon^{\cos\theta\epsilon^{-2i\theta}} \} = \frac{\pi}{2}\left\{ f\epsilon^{\frac{1}{2}} + \frac{\epsilon^{\frac{1}{2}}}{4} f'\epsilon^{\frac{1}{2}} \right\} \quad \ldots \quad (139).$$

$$\int_0^{\infty} dx \frac{\epsilon^{2irx} f(2\sin x\epsilon^{i(\frac{\pi}{2}+x)}) + \epsilon^{-2irx} f(2\sin x\epsilon^{-i(\frac{\pi}{2}+x)})}{(a^2+x^2)(b^2+x^2)(c^2+x^2)\ldots(e^2+x^2)}$$

$$= \frac{\pi}{a(a^2-b^2)(a^2-c^2)\ldots(a^2-e^2)} f(\epsilon^{-2a}-1)\,.\,\epsilon^{-2ar}$$

$$+ \frac{\pi}{b(b^2-a^2)(b^2-c^2)\ldots(b^2-e^2)} f(\epsilon^{-2b}-1)\,.\,\epsilon^{-2br} + \ldots \quad \ldots \quad (140).$$

Similarly we may find

$$\int_0^{\infty} x\,dx \frac{\epsilon^{2irx} f(2\sin x\,\epsilon^{i(\frac{\pi}{2}+x)}) - \epsilon^{-2irx} f(2\sin x\epsilon^{-i(\frac{\pi}{2}+x)})}{(a^2+x^2)(b^2+x^2)(c^2+x^2)\ldots(e^2+x^2)} \quad \ldots \quad (141).$$

$$\int_0^{\pi} d\theta \frac{\epsilon^{ir\theta} f\left(2\sin\frac{\theta}{2}\epsilon^{i(\frac{\pi+\theta}{2})}\right) + \epsilon^{-ir\theta} f\left(2\sin\frac{\theta}{2}\epsilon^{-i(\frac{\pi+\theta}{2})}\right)}{1-2\alpha\cos\theta+\alpha^2} = \frac{2\pi f(\alpha-1)\,.\,\alpha^r}{1-\alpha^2} \quad (142).$$

Similarly we may obtain:—

$$\int_0^{\pi} d\theta \sin\theta \frac{\epsilon^{ir\theta} f\left(2\sin\frac{\theta}{2}\epsilon^{i(\frac{\pi+\theta}{2})}\right) - \epsilon^{-ir\theta} f\left(2\sin\frac{\theta}{2}\epsilon^{-i(\frac{\pi+\theta}{2})}\right)}{1-2\alpha\cos\theta+\alpha^2} \quad \ldots \quad (143).$$

$$\int_0^{\infty} dx \frac{\epsilon^{2irax} f(2\sin ax\epsilon^{i(\frac{\pi}{2}+ax)}) + \epsilon^{-2irax} f(2\sin ax\epsilon^{-i(\frac{\pi}{2}ax)})}{(\alpha^2+x^2)(\beta^2+x^2)(\gamma^2+x^2)\ldots(\lambda^2+x^2)\,.\,\cos ax} \quad \ldots \quad (144).$$

$$\int_0^{\infty} x\,dx\,.\,\frac{\epsilon^{2irax} f(2\sin ax\epsilon^{i(\frac{\pi}{2}+ax)}) + \epsilon^{-2irax} f(2\sin ax\epsilon^{-i(\frac{\pi}{2}ax)})}{(\alpha^2+x^2)(\beta^2+x^2)(\gamma^2+x^2)\ldots(\lambda^2+x^2)\,.\,\sin ax} \quad \ldots \quad (145).$$

$$\int_0^{\frac{\pi}{2}} d\theta\,.\,\frac{f(\cos\theta\epsilon^{i\theta}) + f(\cos\theta\epsilon^{-i\theta})}{x^2\cos^2\theta + \alpha^2\sin^2\theta} = \frac{\pi}{ax} f\frac{x}{x+a} \quad \ldots \quad (146).$$

$$\int_0^{\infty} dx \frac{f(\epsilon^{2x}) - f(\epsilon^{-ix})}{x} = \pi i(f(1) - f(0)) \quad \ldots \quad (147).$$

These formulæ may be greatly extended. I add a few examples of their application to particular cases:—

$$\int_0^{\frac{\pi}{2}} d\theta \frac{\epsilon^{\cos^2\theta\cos 2\theta}\cos(\cos^2\theta\sin 2\theta)}{x^2\cos^2\theta + \alpha^2\sin^2\theta} = \frac{\pi}{2ax}\epsilon^{\frac{x^2}{(x+a)^2}} \quad \ldots \quad (148).$$

$$\int_0^{\frac{\pi}{2}} \frac{d\theta \log_e(1+2\alpha\cos^2\theta\cos 2\theta+\alpha^2\cos^4\theta)}{x^2\cos^2\theta+a^2\sin^2\theta} = \frac{\pi}{ax}\log_e\frac{(x+\alpha)^2+\alpha x^2}{(x+a)^2} \quad (149).$$

$$\int_0^{\infty}\frac{d\theta}{\theta}\cdot\frac{\sin\theta}{1+2\alpha\cos\theta+\alpha^2} = \frac{\pi}{2}\cdot\frac{1}{1+\alpha} \quad \ldots \ldots \ldots \quad (150).$$

$$\int_0^{\infty} d\theta \cdot \frac{e^{\alpha\cos\theta}\sin(\alpha\sin\theta)}{\theta} = \frac{\pi}{2}(e^{\alpha}-1) \quad \ldots \ldots \ldots \quad (151).$$

$$\int_0^{\infty}\frac{d\theta}{\theta}\sqrt{\sqrt{(1+2\alpha\cos\theta+\alpha^2)}-(1+\alpha\cos\theta)} = \frac{\pi}{\sqrt{2}}(\sqrt{1+\alpha}-1) \quad . \quad (152).$$

In my last paper I gave the integral

$$\int_0^{\pi} d\theta \frac{\sin r\theta}{1-2\alpha\cos\theta+\alpha^2} \quad \ldots \ldots \quad (132).$$

It is obvious that by a similar process we can find

$$\int_0^{\pi} d\theta \frac{\cos r\theta}{1-2\alpha\sin\theta+\alpha^2} \quad . \quad (153). \qquad \int_0^{\pi} d\theta \frac{\sin r\theta}{1-2\alpha\sin\theta+\alpha^2} \quad . \quad (154),$$

remembering that $1-2\alpha\cos\left(\frac{\pi}{2}+\theta\right)+\alpha^2 = 1+2\alpha\sin\theta+\alpha^2$.

Also since

$$\int\frac{d\theta\cos(r+1)\theta}{(a+b\cos\theta)^n} = \frac{2}{b}\int\frac{d\theta\cos r\theta}{(a+b\cos\theta)^{n-1}} - \frac{2a}{b}\int\frac{d\theta\cos r\theta}{(a+b\cos\theta)^n} - \int\frac{d\theta\cos(r-1)\theta}{(a+b\cos\theta)^n} \quad \ldots \quad (155),$$

$$\int\frac{d\theta\sin(r+1)\theta}{(a+b\cos\theta)^n} = \frac{2}{b}\int\frac{d\theta\sin r\theta}{(a+b\cos\theta)^{n-1}} - \frac{2a}{b}\int\frac{d\theta\,.\sin r\theta}{(a+b\cos\theta)^n} - \int\frac{d\theta\sin(r-1)\theta}{(a+b\cos\theta)^n} \quad \ldots \quad (156),$$

$$\int\frac{d\theta\sin(r+1)\theta}{(a+b\sin\theta)^n} = \frac{2}{b}\int\frac{d\theta\cos r\theta}{(a+b\sin\theta)^{n-1}} - \frac{2a}{b}\int\frac{d\theta\,.\cos r\theta}{(a+b\sin\theta)^n} + \int\frac{d\theta\sin(r-1)\theta}{(a+b\sin\theta)^n} \quad \ldots \quad (157),$$

$$\int\frac{d\theta\cos(r+1)\theta}{(a+b\sin\theta)^n} = \frac{2a}{b}\int\frac{d\theta\sin r\theta}{(a+b\sin\theta)^n} - \frac{2}{b}\int\frac{d\theta\sin r\theta}{(a+b\sin\theta)^{n-1}} + \int\frac{d\theta\cos(r-1)\theta}{(a+b\sin\theta)^n} \quad \ldots \quad (158),$$

it is manifest that:—

$$\int_0^{\pi} d\theta\frac{\cos r\theta}{(1-2\alpha\cos\theta+\alpha^2)^n} \quad . \quad (159), \qquad \int_0^{\pi} d\theta\frac{\sin r\theta}{(1-2\alpha\cos\theta+\alpha^2)^n} \quad . \quad (160),$$

$$\int_0^\pi d\theta \cdot \frac{\sin r\theta}{(1-2\alpha\sin\theta+\alpha^2)^n} \cdot (161), \quad \int_0^\pi d\theta \frac{\cos r\theta}{(1-2\alpha\sin\theta+\alpha^2)^n} \cdot (162),$$

may be reduced to integrals 132, 153, 154, and other known forms, and that consequently (resolving into partial fractions)

$$\int_0^\pi d\theta \frac{\cos r\theta}{(1-2\alpha\cos\theta+\alpha^2)^m(1-2\beta\cos\theta+\beta^2)^n\ldots(1-2\lambda\cos\theta+\lambda^2)^r} \ (163),$$

$$\int_0^\pi d\theta \frac{\sin r\theta}{(1-2\alpha\cos\theta+\alpha^2)^m(1-2\beta\cos\theta+\beta^2)^n\ldots(1-2\lambda\cos\theta+\lambda^2)^r} \ (164),$$

$$\int_0^\pi d\theta \frac{\cos r\theta}{(1-2\alpha\sin\theta+\alpha^2)^m(1-2\beta\sin\theta+\beta^2)^n\ldots(1-2\lambda\sin\theta+\lambda^2)^r} \ (165),$$

$$\int_0^\pi d\theta \cdot \frac{\sin r\theta}{(1-2\alpha\sin\theta+\alpha^2)^m(1-2\beta\sin\theta+\beta^2)^n\ldots(1-2\lambda\sin\theta+\lambda^2)^r} \ (166),$$

may be ascertained. We may also find

$$\int_0^\pi \log_\epsilon{}^2\left(2\cos\frac{\theta}{2}\right)d\theta = \frac{\pi^3}{12} \quad . \ . \ . \ . \ . \quad (167).$$

$$\int_1^\infty \frac{dx}{x^2}\left(\log_\epsilon\frac{x}{\epsilon}\right)^3\left(\log_\epsilon x\right)^n = (n^4+3n^3+5n^2+2n)\Gamma(n) \quad . \quad (168).$$

If we expand the denominator of the integral

$$\int_0^\pi d\theta \frac{\cos\rho\theta}{1-2\alpha\cos\theta+\alpha^2} \text{ where } \rho=\frac{m}{n} \quad . \ . \ . \ . \quad (169),$$

and integrate the terms in succession, we shall have to determine the integral series of the form

$$\frac{\alpha}{n+m}+\frac{\alpha^2}{2n+m}+\frac{\alpha^3}{3n+m}+\ldots$$

which may always be found, when the values of (m) and (n) are assigned, from the expanded form of $\log_\epsilon(1+x)$ by the method of summation of the equidistant terms of series. Similar reasoning will apply to

$$\int_0^\pi d\theta \frac{\sin\rho\theta}{1-2\alpha\cos\theta+\alpha^2} \quad . \quad (170). \qquad \int_0^\pi d\theta \cdot \frac{\cos\rho\theta}{1-2\alpha\sin\theta+\alpha^2} \quad . \quad (171).$$

$$\int_0^\pi d\theta \frac{\sin\rho\theta}{1-2\alpha\sin\theta+\alpha^2} \quad . \ . \ . \ . \ . \ . \ . \quad (172).$$

This method of summing the equidistant terms of series may be applied to the determination of the values of other integrals, as for instance

$$\int_0^\pi \frac{d\theta\,\theta\sin\theta\cos^{2r}\theta}{1-k\cos^2\theta}\ .\ (173),\qquad \int_0^\pi d\theta\log_e\cos\theta\frac{\cos 2\theta+a\cos 6\theta}{1-2a\cos 8\theta+a^2}\ .\ (174),$$

$$\int_0^1 \frac{dx\,.\,x^m}{1-kx^n}\quad .\quad .\quad (175),\text{ with many others.}$$

(Received February 4, 1881.)

I have received permission to write down formula (132) thus amended:—

$$\int_0^\pi \frac{\sin r\theta d\theta}{1-2a\cos\theta+a^2}=\frac{1}{1-a^2}\left(a^r-\frac{1}{a^r}\right)\log_e\frac{1-a}{1+a}$$

$$+\frac{2}{1-a^2}\left\{\left(a^{r-1}-\frac{1}{a^{r-1}}\right)+\frac{1}{3}\left(a^{r-3}-\frac{1}{a^{r-3}}\right)+\ldots\frac{1}{r-1}\left(a-\frac{1}{a}\right)\right\}.$$

III. "*Polacanthus Foxii*, a large undescribed Dinosaur from the Wealden Formation in the Isle of Wight." By J. W. HULKE, F.R.S. Received January 3, 1881.

(Abstract.)

A description of the remains of a large Dinosaur, discovered in 1865 by the Rev. W. Fox, in a bed of shaly clay between Barnes and Cowleaze Chines, in the Isle of Wight. Head, neck, shoulder-girdle, and foreribs were missing, but the rest of the skeleton was almost entire. Some of the præsacral vertebræ recovered show a double costal articulation. In the trunk and loins the centrum is cylindroid, relatively long and slender, with plano-concave, or gently biconcave ends. Several lumbar centra are ankylosed together, and the hindmost to the sacrum. The sacrum comprises five relatively stout and short ankylosed centra of a depressed cordiform cross-sectional figure. The front sacral vertebræ have a stout short centrum.

The limb bones are short, their shafts slender, and their articular ends very expanded. The femur has a third trochanter, and the distal end of the tibia has the characteristic dinosaurian figure.

The back and flanks were stoutly mailed with simple, keeled, and spined scutes, and the tail was also sheathed in armour.

The animal indicated by these remains was of low stature, great strength, and probably slow habits. It is manifestly a Dinosaur, and is considered to be very nearly related to *Hylæosaurus*.

IV. "On Harmonic Ratios in the Spectra of Gases." By ARTHUR SCHUSTER, Ph.D., F.R.S. Received January 10, 1881.

It would be a matter of the greatest importance if we could discover an empirical law connecting together the different periods of vibration in which we know one and the same molecule to be capable of swinging. According to the most simple supposition the vibrations might be harmonical overtones of one fundamental note. Various attempts have been made to prove that such indeed is the case, and that the wave-lengths of different spectral lines bear to each other the ratio of two comparatively small integer numbers. M. Lecoq de Boisbaudran and Professor Johnstone Stoney, especially, have discussed this question; the wave-lengths used by the former do not possess the accuracy necessary for a final settlement of the point, but Professor Stoney has, in the case of hydrogen, shown that three out of the four lines in the visible part of the spectrum have wave-lengths, which, to a high degree of accuracy, are in the ratios of 20 : 27 : 32.

I have occupied myself at various times during the last ten years with this question, and have naturally accumulated a large quantity of material. About three years ago, however, I came to the conclusion that only a systematic investigation could lead to a decisive result. In any spectrum containing a large number of lines, it is clear that, owing to accidental coincidences, we shall always be able to find ratios which agree very closely with the ratios of small integer numbers. We can, however, by means of the theory of probability, calculate the number of such coincidences which we might expect to find on the supposition that no real law exists, and that all the lines are distributed at random throughout the whole range of the visible spectrum. If, on calculating out all fractions which can be formed in a spectrum by any pair of lines, the number of ratios, agreeing within certain limits with ratios of integer numbers, greatly exceeds the most probable number, we should have reason to suppose that the lines are not distributed at random, but that the law suggested by Messrs. Lecoq de Boisbaudran and Stoney is a true one.

I have been engaged during the last three years in discussing some of the spectra in the manner indicated, and I now wish to lay the results of the investigation before the Royal Society. I took the spectra of the following elements; the numbers in brackets indicate the number of lines for each body:—

Magnesium	(7)
Sodium	(10)
Copper	(15)
Barium	(26)
Iron	(149)

I have only taken such lines as are found on Ångström's map, and I have compared the ratios of any two lines with the ratios of integer numbers smaller than 100. These latter ratios were calculated out to six decimal places, and arranged in order of magnitude in a table, to which I shall refer as the Auxiliary Table. I have adopted two methods of comparison. The first is best explained by an example. The wave-length of the less refrangible of the two yellowish-green sodium lines divided by the wave-length of the less refrangible of the two yellow lines gave the ratio .... ·964760

On referring to the Auxiliary Table we find that this ratio lies between .... $55 \div 57 =$ ·964912

and .... $82 \div 85 =$ ·964706

The difference between these two fractions being.......... ·000206

The difference of the fraction in the sodium spectrum with the nearest fraction of integer number is .... ·000054

The ratio of these two differences $54 \div 206$ is found ..... ·262

Similar ratios were formed for all possible fractions in the sodium spectrum. Now, if the lines in spectra are distributed at random, we should expect the ratio of the two differences to range indiscriminately between 0 and ·5; the mean of all of them coming near ·25. If, on the other hand, the law of harmonic ratios is a true one, we should expect a greater number of small fractions, and hence the mean should be smaller than ·25. The results are given in Table I. The second column gives the numbers of fractions for each spectrum, and the third the mean values obtained, which, as mentioned, ought to be near ·25, if the lines are distributed at random.

Table I.

Element.	Number of fractions.	Mean value of ratios.	P = ±.
Magnesium	18	·2626	·0229
Sodium	40	·2399	·0154
Copper	101	·2430	·0097
Barium............	303	·2592	·0056
Iron	10404	·2513	·0010
Mean	10866	·2514	

Nothing could be more decisive against the law of harmonic ratios than this table; three out of the five elements considered, including the two containing the greatest number of lines, give a mean value greater than ·25.

In order to see how near to this value we should expect the mean to come if no law connects the different lines, I have given the probable

deviation from ·25 in the fourth column. The term probable in probable deviation is here used in the same sense as in "probable error." It has been calculated by means of the approximate formula—

$$p=\frac{2}{\sqrt{\pi}}\,\text{erfc.}\sqrt{\frac{3s}{2}}\,\frac{\delta}{a}$$

where $a=\cdot 25$,

$s=$the number of lines in each spectrum,

p is the probability that the mean value lies between $\pm\,\delta$; for p equal to one-half, δ is the probable deviation.

It will be noticed that the actual deviation never differs much from the probable one, but that it is greater for the two elements having the greatest number of lines. If, therefore, any deduction is to be drawn from the preceding table, it is that the ratios formed by two given lines rather seem to avoid harmonic ratios.

The method just explained, and which has given us such decidedly negative results, I believe to be very well adapted for the discussion of spectra which have a comparatively small number of lines; but the iron spectrum may be examined by a more direct and complete method. We may directly calculate how many fractions ought to agree within certain small limits with harmonic ratios if no law exists, and counting how many do thus coincide. I have found, for instance, twenty-eight pairs of lines which coincide within limits so narrow that they can be easily due to errors of measurements with fractions, the denominator and numerator of which are both smaller than 10. This number might appear large at first sight, and some support for the law of harmonic ratios might be derived from it. But the calculation gives the larger number 32 as the one we ought to expect, if all the lines were distributed at random; so that here, also, the fractions seem to avoid rather the harmonic ratios.

A little difficulty is experienced in fixing the limits within which we may consider a coincidence to have taken place. They must depend, of course, on the accuracy which we assign to Ångström's measurements.

I thought it best to work out the results with two different limits, one of which was half as large again as the other. We gain a decided advantage in classifying the results for two limits. It is in fact equivalent to using a third method of discussion, for supposing the spectral lines to be distributed at random, the number of coincidences found should be proportioned to the limits chosen. If, on the other hand, the law of harmonic ratios is correct, the narrower limit should relatively show the greater number of coincidences. The limits taken were—

$$\pm\,\cdot 0000505$$

and

$$\pm\,\cdot 0000755.$$

so that two lines were said to have the ratio of 3 : 4, for instance, if in the first case the ratio lay between ·7500505 and ·74995195, and similarly for the second limit.

If the measurement of the least refrangible line is correct, an error of 1 in 20,000 made in the measurement of the most refrangible line would correspond to the narrower limit.

The results are given in Table II. In the first row all fractions were taken into account the denominator of which is smaller than 10; in the second row, the denominator is between 10 and 20, and so on, for the other rows. The columns headed "calculated," give the number of coincidences which we should expect on the supposition that the lines are distributed at random. The formula employed will be proved in the Appendix.

Table II.

	Limits ± ·0000505.		Limits ± ·0000755.	
	Observed.	Calculated.	Observed.	Calculated.
0—10........	48	52	64	77
10—20........	180	206	250	308
20—30........	329	363	469	544
30—40........	478	521	664	779
40—50........	625	679	912	1015
50—60.......	777	837	1163	1251
60—70........	886	968	1318	1447
70—80........	924	896	1337	1340
80—90........	667	629	989	940
90—100........	253	241	393	361
Total.......	5167	5392	7559	8062

At first sight the result seems again decidedly against the theory of harmonic ratios. For all fractions with denominator smaller than 70, the calculated coincidences are in excess of the observed ones. There seems, however, to be a greater number of ratios than we should expect, which agree nearly with fractions, the denominators of which lie between 70 and 100.

If we compare the results given for the two different limits, we find that the smaller limit gives results decidedly more favourable to the theory than the larger ones, and that, as has been explained, is an important fact which cannot be left out of account. In the following Table (III), I have compared the number of coincidences for the smaller limit with those calculated from the larger one, on the supposition that the coincidences are proportional to the limits, as they ought to be if no connexion exists between different lines of the same spectrum. It will be seen, that with the exception of two cases, one

of which is very insignificant, the number of coincidences for the smaller limits is in excess.

Table III.

	Observed for smaller limit.		Calculated from larger limit.
0—10..........	48	......	43
10—20..........	180	......	167
20—30..........	329	......	314
30—40..........	478	......	444
40—50..........	625	......	610
50—60..........	777	......	778
60—70..........	886	......	882
70—80..........	924	......	894
80—90..........	667	......	662
90—100..........	253	......	263
	5167		5057

The fact that the number of coincidences, though falling short of the calculated values for both limits, is relatively greater for the smaller, suggests the possibility that still narrower limits might give results which are still more favourable to the theory of harmonic ratios. This indeed is the case. I have counted for all fractions, the denominator of which is smaller than 30, the number of coincidences for a series of 8 limits. The results are embodied in Table IV, and show that there is a tendency of the fractions to aggregate into the compartments for smaller limits. With the exception of the first and last numbers, there is a gradual decrease of coincidences as we recede from the harmonic ratios.

Table IV.

Limits ± ·0000.		Number of coincidences.
000—095	....	71
095—195	....	85
195—205	....	78
295—395	....	70
395—495	....	68
495—595	....	66
595—695	....	56
695—755	....	41

We have now to reconcile two apparently opposite results of our calculations. On the one hand it was found that the coincidences with harmonic ratios are fewer than we should expect from the theory

of probability, and on the other hand the results obtained with different limits showed that the smallest always gave the most favourable result. The regularity with which this latter fact appears in Tables III and IV, proves it not to be accidental, and if not accidental, it can only mean that the law of harmonic ratios is at least partially a true law.

The following explanation has occurred to me as possibly accounting for the facts. We may suppose the harmonic ratios really to exist in appreciable numbers, but to be chiefly confined to fractions, the denominator and numerator of which are larger than those we have taken into account. The fractions, for instance, formed by integers between 100 and 200, if arranged in order of magnitude in our auxiliary tables, would fall generally about midway between the fractions formed by the smaller numbers. Any coincidence with the fractions formed by the higher numbers would reduce the number of possible coincidences with the fractions formed by the smaller numbers, and hence we should have the effect which actually exists, of a number of coincidences smaller than that given by the theory of probability. If, now, in addition to these coincidences with fractions formed by higher numbers, we should have a small quantity of real coincidences with the fractions which we have taken into account, the increased quantity of coincidences for small limits over those of larger limits, would be explained.

This explanation might be supported by the fact that, for fractions formed by numbers between 70 and 100, the coincidences observed are more numerous than those calculated on the supposition that the lines are all distributed at random. It must, however, be remarked that a similar effect might be produced, if any unknown law existed, connecting the lines together, a law which in special cases reduced itself to a law of harmonic ratios.

That some law hitherto undiscovered exists I have no doubt, for just in the cases where we have reason to suppose that different lines belong to one system of vibration, we cannot find any coincidences with harmonic ratios. The lines of sodium, for instance, are all double; yet in the set of lines given by Thalèn the two components approach each other much more rapidly as we pass to the more refrangible end of the spectrum than they would if the lines were connected together by the harmonic law. In the additional sets described by Professors Liveing and Dewar no regularity exists in the distance of the two components.

A similar remark applies to the four triplets of magnesium lines. The triplets resemble each other in so far as the middle line is always nearest to the most refrangible line; but the resemblance is only a general one, and there is no absolute relation between the relative distances in each triplet.

Taking all these considerations into account, the following seems to me to be a fair summary of my results for the iron spectrum:—

1. *There is a real cause acting in a direction opposed to the law of harmonic ratios, so far as fractions formed by numbers smaller than seventy are concerned.*

2. *After elimination of the first cause a tendency appears for fractions formed by two lines to cluster round harmonic ratios.*

3. *Most probably some law hitherto undiscovered exists, which in special cases resolves itself into the law of harmonic ratios.*

The subject is of sufficient importance to make further investigation desirable. We might, for instance, confirm the laws which we have found to hold in the iron spectrum by treating in the same way some other spectrum having many lines, as those of manganese of calcium. But it seems to me to be more promising to increase the accuracy of measurement in the special cases where harmonic ratios have been found. There are, for instance, two lines in the iron spectrum which are in the ratio of 2 : 3. By using a diffraction grating we might test this coincidence to a great degree of accuracy by seeing how far the more refrangible line in the third spectrum coincides with the less refrangible line in the second spectrum. Account, of course, must be taken of atmospheric refraction; reflecting surfaces only ought to be used. I hope to try this plan before long, but in order that others might have the same opportunity, I append a list of all lines which are nearly in the ratio of some fraction formed by integer numbers smaller than ten.

Ångström's numbers corrected for atmospheric refraction are used. The table explains itself, but it is perhaps wise to remark again that the number of these coincidences is not larger than one would expect by the theory of probability, and that therefore all of them may prove to be accidental.

	Fraction.	Calculated.	Observed.	Difference.
6009·33	2 : 3	4006·22	4006·03	−·19
5603·40	3 : 4	4202·55	4202·74	+·19
5598·88	3 : 4	4199·16	4199·16	±·00
5576·59	3 : 4	4182·44	4182·53	+·13
5340·82	3 : 4	4005·62	4006·03	+·41
6302·47	4 : 5	5041·98	5041·67	−·31
5430·47	4 : 5	4344·38	4344·33	−·05
5340·82	4 : 5	4272·66	4272·54	−·12
5264·07	4 : 5	4211·26	4211·09	−·17
5227·85	4 : 5	4182·28	4182·53	+·25
5193·27	4 : 5	4154·62	4154·96	+·34
6231·66	5 : 6	5193·05	5193·27	+·22
6003·91	5 : 6	5003·26	5003·50	+·24
5987·99	5 : 6	4989·99	4989·85	−·14

	Fraction.	Calculated.	Observed.	Difference.
5169·93	5 : 6	4308·28	4308·45	+ ·17
6137·53	5 : 7	4383·95	4384·04	+ ·09
5430·47	6 : 7	4654·69	4654·9	+ ·2
5284·27	6 : 7	4529·37	4529·35	− ·02
5140·20	6 : 7	4405·89	4405·50	− ·39
6137·53	7 : 8	5370·34	5370·66	+ ·32
6066·38	7 : 8	5308·08	5308·11	+ ·03
5383·99	7 : 8	4710·99	4710·80	− ·19
5271·06	7 : 8	4612·18	4612·08	− ·10
5984·66	7 : 9	4654·74	4654·9	+ ·2
5447·57	7 : 9	4237·00	4236·75	− ·25
5372·16	7 : 9	4178·35	4178·28	− ·07
5341·85	7 : 9	4154·77	4154·96	+ ·19
5328·90	7 : 9	4144·70	4144·30	− ·40
6003·91	7 : 10	4202·74	4202·74	± ·00
6009·33	8 : 9	5341·63	5341·85	+ ·22
5616·24	8 : 9	4992·24	4991·89	− ·35
4633·47	8 : 9	4118·64	4118·94	+ ·30
6192·45	9 : 10	5573·21	5573·35	+ ·14
5603·40	9 : 10	5043·06	5042·71	− ·35
4692·02	9 : 10	4222·82	4222·88	+ ·06
4707·92	9 : 10	4237·13	4236·75	− ·38
4787·23	9 : 10	4308·51	4308·45	− ·06
5233·71	9 : 10	4710·34	4710·80	+ ·46

Appendix.

The problem which we have to solve may be stated as follows:—

Given a certain number of quantities distributed at random between two fixed limits; form the ratios between every pair of them, and find the expectancy for the number of these ratios which shall within certain small limits agree with a given fraction. In the first place, we remark that without detriment to the generality of the problem, we may assume the lower of the limits within which all the quantities are lying to be unity; for if it is not, we may by means of a common multiplier to all quantities reduce it to unity.

Let α be the given fraction with which all the ratios are to be compared, and let A be the higher limit which none of the quantities shall exceed. Assume at first A to be smaller than the square of the reciprocal of α. Divide the range A to 1 into two compartments; the first from A to $\frac{1}{\alpha}$, and the second from $\frac{1}{\alpha}$ to 1. Let there be t quantities which I shall call $a_1, a_2, \ldots$ in the first compartment; and let there be r quantities $b_1, b_2, \ldots$ in the second compartment. None of the quantities within one compartment can form amongst themselves ratios which shall be closely coincident with α: say $\alpha \pm \delta$.

If there is only one quantity, a, in the first compartment, b is given by the equation—

$$\frac{b}{a}=\alpha\pm\delta$$

$$b=\alpha a\pm\delta a.$$

Hence, if there is only one b which can range between $\frac{1}{\alpha}$ and 1, and must lie between $a\ (\alpha+\delta)$ and $a\ (\alpha-\delta)$, if there is a coincidence with the given fraction, the probability of such a coincidence is

$$\frac{2\alpha a}{1-\alpha}\delta.$$

If there is more than one quantity, a, in the first compartment, we observe that these quantities may lie so near together that one and the same b can have, within the limits within which we count coincidences, the required ratio with more than one of the quantities, a. If these quantities, however, are not sufficiently close together to admit of any such double coincidence, the probability that one b should have the required ratio with one a is

$$\frac{2\alpha}{1-\alpha}\delta(a_1+a_2+\ldots a_t).$$

Call the sum in brackets s_t.

If we drop the limitation that b should not possibly have at the same time the required ratio with more than one a, the expression just found will not any more represent the probability of a single coincidence, but it will represent the expectancy for the coincidences. For in the most general case there is a certain range, A, within which b may lie in order to have the required ratio with one of the quantities, a; there is a range, A_2, within which a double coincidence will happen, and so on: hence the expectancy for the coincidences is

$$(A_1+2A_2+3A_3+\ldots)\div\frac{1}{\alpha}-1,$$

but the expression in brackets is always equal to

$$2\delta s_t,$$

and hence the expression which we have found will represent the expectancy if there is only one quantity, b; for r quantities it is

$$\frac{2\delta\alpha}{1-\alpha}rs_t.$$

We have hitherto supposed that the quantities, a, are at given fixed places, or that s_t has a certain given value. Let $p_t ds_t$, be the proba-

bility that the sum of all the quantities, α, shall lie between s_t and s_t+ds_t, then the whole expectancy is

$$r\frac{2\delta\alpha}{1-\alpha}\int_{\frac{t}{\alpha}}^{tA} p_t s_t ds_t.$$

The integral represents the expectancy for the sum of t quantities all equally probable between $\frac{1}{\alpha}$ and A, and this expectancy we know to be—

$$\frac{1}{2}\left(A+\frac{1}{\alpha}\right)t.$$

Hence the required expression is—

$$rt\delta\frac{A\alpha+1}{1-\alpha}.$$

In the actual case neither r nor t are given, we only know their sum n; hence we must add up a number of expressions of the form we have found, varying r and t, and multiplying each with the probability that the particular distribution actually exists.

The probability that there should be t out of n values in the first compartment is—

$$\frac{n!}{t!\,(n-t)!}\frac{(\alpha A-1)^t(1-\alpha)^s}{\alpha^n(A-1)^n},$$

giving t successively all values from 1 to $n-1$ we find for the whole expectancy—

$$\delta\frac{A\alpha+1}{(1-\alpha)}\frac{1}{\alpha^n(A-1)^n}\sum_{t=1}^{t=n-1}\frac{t(n-t)n!}{t!\,(n-t)!}(\alpha A-1)^t(1-\alpha)^{n-t},$$

and adding up under the summation sign, the expression reduces to—

$$\frac{n\,.\,n-1}{(A-1)^2}\frac{A^2\alpha^2-1}{\alpha^2}\delta,$$

which is the complete expectancy.

We have assumed that A is not larger than the reciprocal of the square of α, and we may now extend the formula to larger values of A.

Imagine a quantity B smaller than $\frac{1}{\alpha^2}$ and larger than $\frac{1}{\alpha}$, and let A gradually increase from B to $\frac{B}{\alpha}$. Divide the whole range A to 1 into two compartments, one from A to B and the second from B to 1, then if a given number of quantities is in each compartment, I can calculate the whole expectancy by knowing :—

1. The expectancy for the coincidences between two quantities in the second compartment.

2. The expectancy for the coincidences between one quantity in the first compartment and one in the second.

Now A is supposed to increase gradually from a value smaller than $\frac{1}{\alpha^2}$ to a value larger than $\frac{1}{\alpha^2}$. As long as it is smaller, the result must be the same as that we have previously obtained, but none of the quantities which enter into the calculation show any discontinuity, as A passes through the value $\frac{1}{\alpha^2}$, and hence the formula cannot change at that point and must be true as far as the value $\frac{B}{\alpha}$, or as B may be in the limit equal to $\frac{1}{\alpha^2}$, we have extended our formula to all values of A smaller than $\frac{1}{\alpha^3}$. It can be further extended in the same way and must in fact be true for all values of A.

V. "Dielectric Capacity of Liquids." By J. Hopkinson, F.R.S.

Received January 6, 1881.

(Abstract.)

These experiments have for object the determination of the refractive indices and the specific inductive capacity of certain liquids, and a comparison of the square of the refractive index for long waves and the specific inductive capacity.

In the following table are given the results obtained for refractive index for long waves deduced by the formula $\mu=\mu_\infty+\frac{b}{\lambda^2}$, the square of μ_∞, and the observed values (K) of the specific inductive capacity.

	μ_∞^2	K
Petroleum spirit (Field's)......	1·922	1·92
Petroleum oil (Field's)........	2·075	2·07
„ (Common)......	2·078	2·10
Ozokerit lubricating oil (Field's)	2·086	2·13
Turpentine (Commercial)	2·128	2·23
Castor oil....................	2·153	4·78
Sperm oil....................	2·135	3·02
Olive oil	2·131	3·16
Neatsfoot oil	2·125	3·07

It will be seen that while for hydrocarbons $\mu^2_{\infty} = K$, for animal and vegetable oils it is not so.

VI. "Note on the Occurrence of Ganglion Cells in the Anterior Roots of the Cat's Spinal Nerves." By E. A. SCHÄFER, F.R.S. Received January 11, 1881.

Ganglion cells are of constant occurrence among the nerve-fibres of the anterior roots of the cat's spinal nerves. They are generally to be found in that part of the anterior root which passes by the ganglion which is seated upon the posterior root. They are not necessarily situated next the ganglion; but are often imbedded in the middle of the anterior root, or found lying along its anterior margin, and therefore as far removed as possible from the ganglion upon the other root. Moreover, they sometimes occur in the anterior root before this has come in contact with the ganglion, just as isolated ganglion cells are occasionally to be found in the posterior root, some little distance on the spinal-cord side of its ganglion. The cells in question, although not in any sense numerous, are to be found in most longitudinal sections of the anterior roots, but they seem to be especially abundant in those of the lower dorsal and lumbar nerves. They resemble on the whole very closely the ganglion cells in the spinal ganglion upon the sensory roots, but it has not hitherto been possible to make out their mode of connexion with the nerve-fibres.

I have sought in vain for ganglion cells in a similar situation in the nerve-roots of man, the dog, the rabbit, and the mouse. The evidence, therefore, appears to be against the existence of any relation between the occurrence of these cells in the anterior root and the phenomenon of sensibility in that root, known as "recurrent sensation," for the latter has been observed in animals in which I have been entirely unable to detect the existence of the cells in question (*e.g.*, the rabbit).

VII. "On the Iron Lines widened in Solar Spots." By J. NORMAN LOCKYER, F.R.S. Received January 13, 1881.

The observations put forward with reserve in my last communication to the Society have now been confirmed.

In the fine spots visible on December 24th, January 1st and 6th, many lines in the spectrum of iron were seen contorted, while others were steady.

The facts are given in the following table:—

	The iron lines indicating motion.		Iron lines, visible in the same field of view, steady.
Dec. 24, 1880..	5403 ·2		
	5404 ·8	....	5410 ·0
	5409 ·0	....	5414 ·5
	5408 ·8		
	5396 ·0		
	5370 ·5		
	5369 ·0	....	5366 ·5
	4919 ·8		
	4918 ·0	....	4923 ·0
	5142 ·2	....	5269 ·8
	5138 ·5	....	5268 ·5
In another part of the same spot—			
	5269 ·8	....	5323 ·5
	5268 ·5	....	5327 ·0 (double).
Jan. 1, 1881...	5323 ·5	....	5269 ·8
	5327 ·0 (double)	....	5268 ·5
Jan. 6, 1881...	4919 ·8		
	4918 ·0	....	4923 ·5

* All lines between λ 5323 ·5 and 5410 ·0 except 5382 ·1.

It is to be noted, that these observations furnish us with an instance of inversion similar to those frequently obtained in our observations of the most widened lines in spots.

The inferences to be drawn from these observations, and those on which we are now continuously engaged, must be matter for future communication. But I cannot resist calling attention to the crucial nature of the evidence, at least as regards iron, in favour of the view first put forward by Sir B. Brodie, whom we have so recently lost, that the constituents of our terrestrial elements exist in independent forms in the sun.†

I have thought it right to send in a record of this work at once, with a view to induce other observers to follow the continually varying phases of the spots during the approaching maximum.

The observations have been made by Mr. H. A. Lawrance, and confirmed by myself in the majority of cases.

* In this spot the D lines indicated motion and *did not retain their parallelism.*

† Lecture delivered before the Chemical Society, June 6, 1867.

Presents, January 6, 1881.

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February 3, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Mountstuart Elphinstone Grant Duff, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Upon the Cause of the Striation of Voluntary Muscular Tissue." By JOHN BERRY HAYCRAFT, M.B., B.Sc., F.R.S.E., Senior Physiological Demonstrator in the University of Edinburgh. Communicated by Dr. KLEIN, F.R.S. Received December 1, 1880.

[PLATE 5.]

The structure of striated muscular tissue has occupied the attention of many histologists, and various, often antagonistic, have been the views held from time to time since Schwann first investigated this difficult subject.

I bring forward with much caution and hesitation any opinions of my own, nor should I venture thus far, did I not consider my views susceptible of direct proof, or disproof, not being matters of mere speculation, which may or may not be true, and which would tend, by their introduction to the literature of the subject, to make confusion worse confounded.

In this paper an attempt will be made to account for many of the observed structural phenomena of muscle on simple laws of geometrical optics, which will, if it be successful, reduce the subject to comparative simplicity. I shall commence by giving a sketch of the views of those physiologists who have especially written upon the structure of muscle. This must not be looked upon as a complete history, for I shall leave out entirely points which do not concern us here.

A Short Historical Sketch of the Views held upon the Structure of Striated Muscle.—The writings of Mr. Bowman form the most im-

portant and brilliant contributions to the literature of this subject, and taking him as a landmark, it is convenient to speak of investigators before or after his time. Among the former Schwann, quoted by Müller ("Physiology," translation by Baly, vol. ii, p. 878), describes the striated voluntary fibre, indicating its shape and size. The cross markings were observed by him, and, indeed, with one or two remarkable exceptions, by all the early observers (Lauth and Wagner, in Müller's "Archiv für Anatomie und Physiologie, und Wissenschaftliche Medicin," pp. 4 and 318, of the year 1835). Schwann, with Bauer, Krause, Müller, Home, Valentin, and Milne Edwards recognised the important fact that each fibre is composed of a number of threads or fibrillæ, packed side by side and joined together by a transparent tenacious fluid (Krause), and, moreover, that these threads or fibrillæ are cross striated, as is the fibre itself. Although Schultze describes the fibrillæ as being uniform filaments, he is alone in this opinion, most of his contemporaries recognising the beaded appearance.* The beaded thread was the cause of some dispute, for the question arose, was it a linear series of globules or a moniliform filament? and the final settlement of this must, indeed, have been a matter of great difficulty to those older *savants*, when we consider the imperfect lenses at their disposal. Krause and others maintained the former view, while Schwann held that which subsequent investigators have shown to be the correct one. The fibrillæ, according to Schwann, present a very regular succession of bead-like enlargements, darker than the very short constrictions which lie between. Thus, before the time of Mr. Bowman, the following important facts had been made out, namely, that the fibre is composed of a bundle of beaded fibrillæ cemented together, and that the fibrillæ are cross striped, giving the whole fibre a like appearance of striation. Erroneous views had often, it is true, been advanced, but these had never received general acknowledgment. Mr. Skey ("Phil. Trans.," 1837), for instance, considered the fibres to be tubes filled with a soluble gluten, the striæ surrounding and binding them together. Leeuwenhoek had a somewhat similar view of the construction of the cross striæ, and Prochaska considered them as depressions caused by the clasping of neighbouring capillaries and thready tissues.

Mr. Bowman communicated to the Royal Society, in 1840, a paper "On the Structure and Movements of Voluntary Muscle," in which he confirmed many of the opinions of his predecessors, adding, at the same time, much of what was fresh to our store of knowledge. He it was who first described the thin elastic membrane (sarcolemma) covering and ensheathing the fibres, showing how easily to demon-

* Consult a drawing by Allen Thomson in illustration of Dr. Martin Barry's paper on the structure of muscular fibrils, "Phil. Mag.," series 4, vol. 5, Plate V, fig. 2.

strate its existence, and giving figures of it, which have been copied into most modern histological works. The nuclei of the sarcolemma he also figured, but what most concerns us is his description of the cross striation. Bowman, I believe, first pointed out that not only can a fibre be split up longitudinally into fibrillæ along certain dark lines which may generally be seen, even in fresh preparations, but that it splits up transversely along the dark stripes. Each fibrilla may, therefore, be split up into tiny segments across the dark striæ. "On the whole, little doubt remains in my mind that the fibrillæ consist of a succession of solid segments or beads connected by intervals generally narrower, and I believe the beads to be light, and the intervals the dark spaces when the fibrilla is in exact focus." His idea of a fibre naturally follows from that just given of a fibrilla, and, quoting again from him, we find "a fibre consists of sarcous elements (so he termed the little segments or beads) arranged and united together endways and sideways, so as to constitute in these directions respectively fibrillæ and discs, either of which may in certain cases be detached as such," and "the dark longitudinal striæ are shadows between fibrillæ, the dark transverse striæ shadows between discs."

It will be seen that in one particular Bowman disagreed with Schwann and the older writers, and at the same time with those of more recent date. According to him, the bead was light and the constriction dark, when the muscle was in exact focus, a description at variance with everyone. In the same paper he mentions this remarkable fact, that on altering the focus the stripes were reversed; he must have examined it—this bears in a most important way on our investigations, to be afterwards described—in the reverse focus of what it is ordinarily figured in. His view of the form, and the splitting of the fibre, was probably correct, for he described the cleavage as occurring in the narrow part, which appeared to him, focussing as he did, to be dark, and indeed it is often difficult to say which it is, whether dark or light, for, as I shall more particularly mention afterwards, the slightest alteration of the focus is sufficient to reverse the appearance of the fibre. Bowman, moreover, accounted for these light and dark parts of the fibrillæ, comparing a muscular filament to a glass rod with alternate swellings and depressions, which, when viewed with transmitted light, gives just the same appearance, and from a study of his paper, although it is here somewhat indefinite, I judge that he concluded the moniliform shape to be a cause of the striping.*

* Bowman, nevertheless, seems to consider the dark stripe of a different structure from the light, not so much from the shading, but from the transverse cleavage. He is not quite definite here, but this is the impression I have gained from a careful perusal of his paper.

Now, this last-named and important discovery of Bowman's has, I believe, completely been lost sight of, for no mention of it can be found in any modern monograph nor in any systematic text-book that I have examined. The striking points in the paper and in the figures he gives, is the splitting up of the fibre into transverse discs and the demonstration of the sarcous elements as before quoted. This, together with the sarcolemma, everyone connects with the name of Bowman. Modern investigators have worked mostly at the cross striping of muscle, and have found it more complicated than Bowman described, owing, no doubt, to the use of better glasses; while he explained the phenomenon as due simply to the shape of the fibres—believing, however, probably that it was due also to structural differences—modern investigators have introduced hypotheses to account for it, which imply differences of structure along the filament. The reason of this is, if I may express an opinion, that his theory has been completely lost sight of, and that it was followed by the discovery of startling facts, which at first sight seemed to set it on one side.

In discussing the views of modern inquirers, I shall not, in all cases, consider them in the order of their priority, and allusion will not be made to much that has been written upon this subject, which, indeed, may safely be put on one side.

The light stripe—dark stripe of Bowman—has been shown by Dobie, Busk, and Huxley to be traversed by a very fine dark band, or rather line, "Querlinie," dividing it into two equal parts. We shall speak of this as Dobie's line, or the dark stripe in the centre of the light. (Fig 1, D, Plate 5.) Then, again, the dark stripe is traversed in its centre by a lighter band called Hensen's stripe.* (Fig. 1, H, Plate 5.) Other bands border this stripe, but as they are certainly not to be seen in all specimens however well prepared, and as we shall presently account for them, they need not trouble us here.

As early as the year 1839, Boeck showed that muscle refracts light doubly, which statement was, however, modified in 1857 by Brücke. The latter examined muscles prepared in alcohol by polarised light, and found that the dark stripe (dark in ordinarily non-polarised light) appeared luminous in the dark field of the microscope, and that the light stripes were dark when the Nicols were crossed. The dark stripes, therefore, appeared to be doubly refracting (anisotropous), and the light stripe singly refracting (isotropous), the fibre consisting of singly and doubly refracting discs alternating one with another. These observations he verified by an examination of the fibre with thin plates of selenite and mica. The views of Brücke have, in their turn, received considerable modifications which will be understood by reference to a diagram. Fig. 2, Plate 5, expresses

* This stripe was also described by Dobie in the "Annals of Natural History" for 1849, and it may be called "Dobie's light stripe."

very well the results of my own observations, which I find, are in accordance with those of other observers. (See the "Handbuch der Physiologie," by Dr. H. L. Hermann, 1879, p. 20.) The black part of the diagram corresponds with the portion of the muscle which singly refracts light (isotropous), while the light shaded parts correspond with the anisotropous substance.

This diagram does not, it will at once be seen, correspond with the views held by Brücke, for the great mass of the light stripe, with Dobie's line in the centre of it is anisotropous, the dark band, as with Brücke, being anisotropous. The most recent view is, then, that both the light and the dark stripes doubly refract light, but that there are bands which lie between them and which are singly refracting. The appearance which partially warrants such a conclusion I have observed, but I shall endeavour to show hereafter how this may most satisfactorily be explained. It will readily be seen how Brücke's view, until quite recently accepted, would drive one to the conclusion that the light and dark stripes represent two different structures alternating in the length of the fibre, and this is corroborated by statements as to the action of staining agents on the tissues.

Muscle is readily stained by picric acid, but is but faintly tinted by carmine, logwood, or eosine, although Ranvier, in his "Traité Technique d'Histologie," states that he has obtained very beautifully stained preparations of insects' muscle, when using Bœhmer's solution of logwood. According to this observer, the dark stripes as well as Dobie's lines are stained, while the rest of the fibre remains colourless. Klein in his "Atlas of Histology" figures the sarcous matter of the dark band clearly tinted, while that of the light stripe is absolutely colourless. The statement will not be far wrong, that everyone at the present time considers the dark and light stripes as representing two different structures, distinct one from another in their physical properties, for the dark stripe is spoken of as possessing a higher refracting power than the light, and chemically, for their compositions have already been hinted at by more than one observer. The dark stripe is looked upon by most as the true contracting part of the fibre, and they are termed the sarcous discs, or "Muskelprismen," "Hauptsubstanz," or masses of "disdiaclasts," and the light stripes as merely connecting matter, "zwischensubstanz," or "Muskelkästchen flüssigkeit." Dobie's line—more especially from the dipping down and attachment of the sarcolemma in insects' muscle at this point—has been looked upon (Krause, "Allgemeine und Mikroscopische Anatomie," section Muskel System, pp. 80—90) as a delicate transverse membrane. This view has received the assent of such microscopists as Klein and Ranvier, but not of Wagener ("Jahresberichte der Anatomie und Physiologie," Hofmann and Schwalbe) and

Rutherford ("Text-book of Physiology," p. 128), who describe Dobie's line as consisting of a row of dots. Engelmann, indeed, describes a row of dots on either side of this line.

Krause would have us believe that the fibre is divided by these membranes into a linear series of little boxes, each box or casket, "Müskelkästchen," containing a dark stripe with (as the membrane lies in the centre of the light stripe) one half of that on either side. Merkel ("Lehrbuch der Gewebelehre," Stuttgart, 1877, p. 83), to make the "Müskelkästchen" self-containing, affirms that the membrane of Krause is double. As to the stripe of Hensen, this is by very many looked upon as still another structure lying in the centre of the dark stripe; it is in many fibres very clearly to be made out, its border being well defined, and in stained preparations (logwood) it has decidedly a lighter tint than the rest of the stripe. Still some (Krause) look upon it as an indication of the highly refracting power of the dark stripe, comparing the appearance with the light centre of an oil globule. The other cross striæ, of which there are many described by some observers, but none at all universally accepted, are, as a rule, considered as indicating further complications in the muscle fibre; indeed, the "Müskelkästchen," by most advanced microscopists, although not $\frac{1}{10000}$ of an inch in length, consists of some ten or twelve different parts. We may postpone, I think, indefinitely the consideration of these details.

While there is great unity as to the appearance of a fibre during a state of rest, the changes which the fibre undergoes when passing into the contracted condition are not at all understood. Not only does one fail to find among histologists agreement as to the changes in appearance, but the interpretations of these are as numerous as the investigators themselves. All are agreed, that during contraction, the fibre as a whole shortens and thickens, but the changes in form which the cross striæ undergo are not understood so well.

Klein, in his "Atlas of Histology," maintains the broadening of both stripes transversely, the dark stripe becoming thinner in the long axis, and the bright stripe more opaque. Ranvier ("Traité Technique d'Histologie," p. 489) states that the only points one can conscientiously observe in the contraction of a living fibre are, that a knot or bulging forms, in which the dark bands approximate, being only separated by Dobie's line. This led him to believe that the dark bands are the true contracting part of the fibre. Ranvier worked especially with osmic acid, fixing the fibres when at rest, and during contraction. W. Krause ("Allgemeine und Mikroscopische Anatomie," p. 92) describes the contraction as follows:—The thickness (in the length of the fibre) of the dark stripe or an isotropous substance remains the same as far as can be seen, while the thickness of the isotropous substance, "Zwischensubstanz" becomes less. From this, he argues that

the substance of the clear stripe, which he considers as fluid "Muskelkästchenflüssigkeit," passes between the little elements of the dark stripe, causing their lateral separation, and therefore broadening and shortening the fibre. Engelmann ("Neue Untersuchungen über die Microskopischen Vorgänge bei der Muskelcontraction," in "Pfluger's Archiv," Band xviii) is certain that the light stripe during complete contraction becomes darker than the dark stripe, and that there is a period as naturally follows from this observation, when the fibre is quite unstriated. The stripes are in fact reversed, the bright one becoming the darker, and *vice versâ*. Both stripes narrow, but especially the bright one. Engelmann advances a theory to account for this, holding that the cause of contraction is the passage of fluid from the isotropous clear stripe into the anisotropous substance; the former shrinks, and the latter swells. Most startling is the view of Merkel ("Hofmann und Schwalbe," vol. i, p. 116), who believes that the dark stripe shifts its position, arranging itself by Dobie's line, while the light stripe passes to the centre.

It is, as will readily be admitted, somewhat difficult to know what to believe, for there is such entire disagreement among physiologists as to simple facts, to say nothing of any conclusions which may be drawn from them. Thinking that there must be some simple clue which would solve the whole problem, I commenced to work at the subject in the summer of 1878. At the onset the clue was discovered, and the substance of the present paper was written by the end of that year, before I had read for the first time the paper of Mr. Bowman's, in the "Transactions" of this Society. My astonishment was indeed great to find in it the first glimmerings of my own opinions, for although the subject had then been worked out but in the rough, and Mr. Bowman had a much simpler problem to deal with, yet undoubtedly he held the same views in the main. My obvious course was therefore entirely to re-write my paper, making every acknowledgment to his already published work. He considered, as far as I can make out, that the light stripe was to be compared with the cement seen in longitudinal fibrillation, between the fibrillæ, yet he looked upon the striæ as being due to the shape of the fibre. From the history of the subject, which has just been given, it will be seen that all observers are not agreed as to the actual appearances of a striped fibre, and especially the changes which occur during contraction, and I hold that they have fallen into great and unwarrantable error in the conclusions (these, indeed, are all contradictory) drawn from these appearances. A fibre has been observed in the field of the microscope, which is marked transversely, as already described, and all modern investigators have concluded that the transverse bands mark the positions of disks (seen on edge) of tissue of different refractive indices and chemical composition, alternating in the long axis of

the fibre. This is, however, purely an assumption which in no way follows.

We can also account for all these cross markings in a way which involves no theory, and requires for its appreciation but a knowledge of most elementary geometrical optics.

If a small fragment of muscle be teazed out in water, salt solution, or almost any other fluid, and examined in the ordinary way, with a power of 300 diameters or more, the important fact may be made out (which is the basis of all my future observations), that the borders of the fibres are not smooth, but undulate, presenting wavy margins. (Fig. 1.)

In the fresh unstained preparation there is a halo around the edge of the fibre which masks the crenulated border, yet by carefully adjusting the mirror so as to obtain oblique light, or by searching for a fibre partly in the shade of another, this may always be made out; in the case of insects' muscle, this is, however, always easy to demonstrate, for the fibres are much coarser, indeed, the appearance has been often figured in the works even of recent histologists. If the preparation be stained by any of the ordinary dyes, perhaps most readily by picro-carmine, the border is in all cases very distinct, and the regularly sinuous margin is unmistakeable. Now, what is the significance of the wavy outline? It is, as will readily be understood, that the fibre is ampullated, the wavy outline being but the optical expression of such a figure. A muscular fibre is then not a smooth cylinder, but is like the turned leg of a chair, or like the transversely ribbed neck of a common water-bottle in shape. If the fibre be broken up into fibrillæ, which is very easy, after maceration in alcohol, these are seen to have just the same characters, indeed, a small bundle of fibrils is most convenient for study. It may be well to remark, that the ultimate fibrillæ often show but little cross marking, and appear almost smooth; that is, however, only due to their small size; a good lens will bring out both points.

The above-described appearances may be observed in all the varieties of muscle that I have as yet examined, *e.g.*, those obtained from man, the dog, cat, rabbit, guinea-pig, mouse, frog, mussel, crab, bee, wasp, Dytiscus, Hydrophilus, common house fly, &c., &c.

The transverse stripings of the fibre are related to and correspond with the inequalities of the surface. (Fig. 1.) The little elevations at the borders correspond, of course, to the little ridges which run round the fibre, while the dips at the borders are the optical expressions of little valleys running between them. In the ordinary position, the dark stripe marks the position of the ridge, and the light stripe lies in the little valleys, as will be seen on reference to fig. 1, Plate 5.

Then, again, Dobie's line (Krause's membrane), which is a faint dark band in the very centre of the bright stripe, runs along the

bottom of the valleys (D in the diagram), and Hensen's stripe in the centre of the dark band, lies on the exact summit of the ridges. (H, fig. 1.)

This position of the stripes in a normal muscular fibre, is the invariable rule, and the idea at once suggested itself, may not the shape of the fibre itself cause the cross stripings?

Any student of natural philosophy would at once affirm that a structureless fibre of such a shape must be cross striped, and a glance at the ribbed neck of the water-bottle on the table will elicit the same answer from any one.

The question we must now determine is, are the appearances seen in the fibre just the same in all their details, as would be produced by a piece of glass, or any other homogeneous transparent substance of the same shape?

Before, however, entering into theoretical grounds, it may be as well to give a full description of what is actually to be seen, for this has yet not been stated.

With a structure of complicated figure, such as the one we are considering, it is obvious that there is no one focus in which it may be described. There is one pretty definite focus for a single speck or thin film, but even when examining a simple cylinder, it is evident that when the borders of it are clear and distinct, the upper surface is slightly out of focus. We shall see, that in the case of the muscle, although there is one position of the lens when the parts are very distinctly seen, and in which they have mostly been described, yet that on slightly altering the focus, the appearance is changed. These changes we must carefully study.

For this purpose we may select the large muscles of the thigh of a rabbit; stretch them ever so little upon a piece of wood, and place them for some days in 50 per cent. alcohol. A high power is required for their examination; I have been in the habit of using a $\frac{1}{24}$-inch of Gundlach, a very perfect lens; a $\frac{1}{10}$-inch will, however, do. A small bundle of fibrils should be selected in preference to a whole fibre for examination.

On focussing it becomes at once apparent that on varying the adjustment ever so little, you may bring into focus the tops of the ridges or the bottoms of the valleys which lie between them. Now this slight alteration is sufficient entirely to change the optical appearances.

First raise the lens until the fibre be out of focus and is only to be seen as a dim streak running across the field, then bring it down until its form and the cross markings are distinctly to be seen (the border is now not quite distinct on a level with the horizontal axis of the fibre). In this position alternating light and dark bands are made out, but no vestiges of Hensen's stripes or Dobie's lines. (Fig. 3, *a*.) The

dark band corresponds with the valley and the light one to the ridge, or crest. This was the focus in which Bowman described his preparations as far as I can gather from the paper. If the lens be now lowered ever so little, the stripes are reversed, a most curious point, which was noticed by Bowman, but afterwards lost sight of. The dark band now corresponds with the ridge, and the bright band with the valley. (Fig. 3, *c*.) This is the focussing in which it is usually described, and in this position Dobie's line and Hensen's stripe are to be seen as a rule in uncontracted fibres.

Between these two positions of the lens there is generally a well-marked intermediate one, which is depicted in fig. 3, *b*. The crests and valleys are both bright and equally so, although the slightest movement of the fine adjuster will make either one or the other the darker; on the slopes, as it were, there are, however, narrow shaded bands, which are shown in fig. 3, *b*. The fibre is now quite clear and distinct, and the longitudinal fibrillation is now best made out—if it can be seen at all—and yet there is no sign of either Hensen's or Dobie's stripes. These being the observed appearances (and they may be verified without very much trouble), I shall calculate theoretically the appearances which a homogeneous fibre of such a shape should present when examined by transmitted light, so as to see whether our observed effects tally with what may be theoretically calculated.

Parallel rays of light pass upwards through the fibre, and in their course are altered in direction (see fig. 4). The substance of the fibre being of higher refrangibility than the fluid in which it is mounted, the thicker parts which correspond to the ridges will act like converging lenses, causing the rays of light to come to a focus (A A′ A″), diverging again. The thinner parts (the valleys) will, on the other hand, act as diverging lenses, causing the rays to spread out, as may be seen on reference to the diagram. Now it is evident that when the objective is arranged to focus those rays which have passed through the fibre and converge over the ridges, at that same position the rays above the valleys will be diverging (see fig. 4). This will produce a difference in the appearance, for the converging rays will give a bright band, while the position of those rays which diverge will appear darker. Alter the focus by screwing the lens up or down, and, provided the fibre can still be seen, this state of matters will be reversed; for after converging, the rays above the position of the ridges will now be diverging, while at the same time those over the valleys will be converging and will appear bright.

The condition seen in fig. 3, *b*, which is intermediate between the low and high focussed picture of the fibre, would be obtained by shifting the lens half-way between these two positions. Hensen's stripe is no doubt due to rays passing through the centre of the ridges suffering little refraction in their course, and thus causing a brightness.

Dobie's line might, of course, be the reverse of this, no rays at this point coming to the eye of the observer; but we shall speak of this more hereafter, when we shall show that there is some reason for suspecting at this point a distinct structure.

Although it is indispensable to account theoretically for these appearances, yet to most persons a simple demonstration will carry more conviction than any proof deduced from the laws of optics, however well they be understood. Instead of showing "what should be," we will study "what is."

For this purpose we will imitate as nearly as possible the figure of a muscular fibre on a small scale, and it shall be made out of a substance of uniform consistence throughout. What appearances will it present on microscopic examination? I have proceeded in the following manner:—A glass rod is heated in a spirit-lamp and plunged into a bottle of Canada balsam; it is then withdrawn, and a little drop of the balsam is allowed to fall on a glass slide, or a thread of it may be laid out on the surface of the glass. Before the drop or thread has solidified it is indented with the milled head of a fine screw, and examined with a power of from twenty to fifty diameters, when cross shadings are to be observed. These are seen, moreover, to correspond with the surface impressions, and not only so, but they are reversed on altering the focus. Hensen's stripe is generally very well seen. The most beautiful and convincing object to study in this connexion is a scale of the Lepisma. These are sold as test objects with many microscopes. They are oval in shape, transparent, and singly refractile throughout, and beautifully ribbed in their length, these ribbings or groovings being indeed so fine that a power of at least 500 diameters will be required to make out those points to be here described. You would think on looking at one of these scales that a piece of muscle was flattened out before you on the field: no rough balsam model, but a perfect illustration taken from the back of a tiny insect.

The appearances it is needless to describe, for they are, almost to the minutest detail, those of a muscular fibre. The bright and dark stripe interchanging with every alteration of focus, Hensen's stripe, and Dobie's line (Krause's membrane) are all to be seen. In the case of the Lepisma scale the line of Dobie is in the centre of a bright band, which is broader than the dark band with Hensen's stripe. This is, of course, the other way in the case of the muscular fibre.

We see, therefore, that a muscular fibre presents just those appearances which a transparent body of uniform texture and of similar shape would possess. However conclusive these proofs may have been, it is well to collect all evidence possible to show that these markings are nothing more than optical effects, to which end a very searching experiment was suggested to me by Professor Tait. It is evident that if these cross bands are seen when parallel, or nearly

parallel, rays of light are passing through the fibre, by using converging or diverging rays the appearance will be altered, and it will be possible by careful adjustment of a lens to cause a total reversal of the striping. If a fibre be carefully focussed and a strong biconcave diverging lens be placed between the stage of the microscope and the mirror, and carefully moved about with the fingers, it will be possible entirely to alter the fibre, causing a total reversal of the cross bands. On withdrawing the lens, of course the fibre resumes its normal appearance. I may mention that several lenses were tried before one was found which would in at all a satisfactory manner show this phenomenon; when successful the experiment is very striking.

In opposition to my view is the one generally accepted, namely, that the cross stripings are produced by differences along the fibre of chemical composition, and refrangibility.

Now, suppose that there were along the fibre two alternating structures, A and B. Let A represent the bright stripe and B the dark stripe. If A has a higher or lower refractive index than B, it is evident that although they were immersed in any number of fluids of refrangibility varying from the lowest to the highest, yet A would always be distinguishable from B, and the striping would always be apparent. Then, again, by placing the fibres in fluids of indices near to that either of A or B, the more striking would be the contrast. If, however, the fibre were homogeneous throughout, the striping being nearly due to the form, then if the fluid and the fibre have the same refractive index all striping will disappear. On Professor Tait's suggestion, I tried a series of fluids formed by mixing, in various proportions, alcohol, whose refractive index is low, with oil of cassia, which is high. In this way I have prepared specimens showing almost no cross striæ, the fibre appearing uniform until after most careful examination.

Dr. Klein has since shown me some muscular fibres of an insect. They were quite smooth and cylindrical, and were unstriated. In these specimens there were, on very close examination, cross lines separated by comparatively wide intervals. It is possible that they represented Dobie's lines.*

But it may well be asked, What about the action of staining agents, such as logwood, which is stated to tint the dark stripe and Dobie's line? Does this not show a difference of structure along the fibres?

Once having the clue it will be understood that just as the unstained

* More recently my friends Messrs. Geddes and Beddard have demonstrated a very curious condition in the muscular fibres of the Echinus, which my views entirely explain. They noticed that in the same fibre some parts were cross striped, while in parts no striation was to be seen. Hearing of my explanation of the markings, they re-examined their specimens (which I have also seen), and found that when the striæ were visible there, and only there, the fibre was ampullated. (See fig. 5.)

fibre will modify and change the direction of rays passing through it, so will also a stained fibre produce what are apparently modifications of the staining effect. It is generally stated that the dark band and Dobie's line are stained by logwood and carmine, while the bright bands remain unaffected; also that Hensen's stripe in the centre of the dark stripe is stained only to a slight degree: whence it follows that if staining action is to be the criterion, this stripe differs in structure from the dark stripe.

We, however, affirm that the whole fibre is stained, and equally stained throughout. The bright band is undoubtedly stained, although it appears not of the deep blue of the dark stripe when coloured by logwood; and this conclusion is drawn not only from an examination of my own specimens, but also from some of great beauty shown to me by Dr. Klein. Why the bright band does not appear of so dark a blue is, that the apparent shading of the latter is added to the blue tint, producing a depth of colour. The most conclusive proof of this is, that one can often reverse the colouring on readjusting the focus, and that Hensen's stripe or the bright part of the dark stripe is only of a faint light-blue, like that of the bright stripe.

Picric acid stains muscle very readily, and colours it throughout. The fibre to the naked eye is yellow and uniformly so, but when examined by the microscope, alternating yellow and shaded yellow bands are to be observed, which reverse their position on changing the focus. With a high focus—when the crests are bright in the unstained preparation—they are of a bright yellow, while the valleys are of a deeper yellow tint.

To show the effects which a fibre of this shape can produce when transmitting monochromatic light, nothing can be more conclusive than the following experiment. A slip of coloured blue glass is held obliquely between the reflector and the stage of the microscope, so that blue rays pass through the fibre. It does not appear of a uniform tint, but beautiful blue stripes are seen corresponding with the crests and valleys, and varying with alterations of focus. If a piece of red glass be substituted for the blue slip, red cross stripes are seen in corresponding places. For this experiment the fresh fibres of insects' muscle should be examined, for, with fine mammalian muscle, the light is not so good, owing to the higher power required. This experiment has been introduced here with the description of stained muscle, not that it can be strictly compared with an ordinary staining process, but simply to show what an influence the fibre's shape must have upon the tinting, supposing, as we do, that this is in reality uniform.

An investigation such as this is beset with many difficulties and fallacies, and I may mention one which befel me in this stage of my work.

I had stained a few muscular fibres of a rabbit with picro-carmine,

and on examination, what was my surprise to find that in some of them the light stripes (valleys) were most brilliantly stained with carmine. I was long puzzled at this, when it was at last discovered that the picro-carmine had dried somewhat on the preparation, and the carmine had mechanically precipitated along the valleys, filling them up. At the end of one or two fibres this precipitation had partially peeled off, showing undoubtedly the true nature of the phenomenon.

I have in my possession very beautiful alcoholic preparations stained with logwood. At first sight, from a study of many of the fibres, one would be led to believe that the bright stripe is wholly unstained, while the dark stripes are of a beautiful violet.

A careful examination, however, reveals the fact that such fibres are broken up transversely, looking like piles of coins, a very common occurrence, especially in preparations that have been long mounted. The coins, lying close to one another, with narrow chinks between, of course revealed transverse unstained tracts, which could well be mistaken for the bright stripe.

More interest and discussion has hitherto accrued to the action of muscle on polarised light, than to the effects of staining reagents. We have seen that much difference of opinion exists: Brücke has maintained that not only is the dark stripe (ridge), as all are agreed, doubly refracting, but that the whole of the light stripe is isotropous. I myself was led to modify this, discovering that on careful focussing with a fibre not at all sheared in its length, the central part of the light stripe was undoubtedly anisotropous. This I have afterwards seen figured, as before mentioned, in Hermann's "Physiology," and have introduced the diagram into fig. 2. It is a point of some practical difficulty to mark exactly the positions of the cross bands while turning the analyser, and thus changing the character of the field. This difficulty has been overcome completely by a suggestion of Professor Tait's, who has helped me much in this part of the work. Very fine emery powder should be sprinkled over the preparation before covering it; for then, on examination, numberless little black specks will be seen in the field. A cross band of a fibre is selected for examination which is exactly opposite one of these little specks, then when you rotate you can definitely affirm, having the little black speck for your guide, what change has occurred.

Rabbits' muscles are very satisfactory objects for examination, as they do not cleave across at all readily. The adductor muscles of the leg should be excised, slightly stretched on a piece of wood, and placed in 50 per cent. alcohol until they split readily into fibrils. They may then be mounted in any ordinary fluid, a pinch of emery powder having been sprinkled over the preparation before covering.

It is necessary to use a power of 800 or 1,000 diameters in the in-

vestigation of mammalian muscle, while in the case of the insect one of 300 diameters is quite enough.

In the living and dead muscular fibre the whole of its substance is doubly refracting. The observations of some modern observers entirely agree with my own, in that, with crossed Nicols, the crests (dark bands) and the centres of the valleys (bright stripes) appear bright and therefore refract light doubly, and that there are two dark bands on the slopes between them. (See fig. 2.) It does not follow, however, that these two dark bands represent tracts of isotropous substance. This is the point at issue. The dark lines between the valleys and ridges which appear when the Nicols are crossed have been interpreted as marking the positions of cross bands of singly refracting substance, but this is a fault of reasoning. If the fibre were smooth and cylindrical it would then follow, but the fibre is not, as we have already insisted. These bands lie just on the sloped parts of the fibre, those sections in fact which are oblique to the passing rays; and the explanation is now quite easy, for the extraordinary ray passing through the fibre is naturally deflected at these parts, and does not reach the eye of the observer. Hence the body appears not to transmit them at all at these parts.

It is not difficult to explain the discrepancies between Brücke's description of the bright stripe and my own.

It is essential to be very scrupulous in the selection of a fibre for examination. It must not be at all twisted, or sheared in the slightest degree, for then the cross stripes are not at right angles to the long axis, and as their width is several times their thickness (in the length) overlapping will to some extent occur. This will certainly lead to very confusing results, and the bright centre of the bright stripe (valley) may well be overlooked. Moreover, the fibre should be slightly stretched and as small as possible.

It has previously been mentioned that in many preparations the fibres split up transversely in a most regular manner, and unless the cover-glass be pressed upon, the little disks remain in position with narrow chinks between them. These chinks will be filled with the isotropous fluid used for mounting, which will lead to very anomalous appearances, and which may perhaps help to account for some of Brücke's statements. These fallacies may be avoided by a study of the fresh fibres of insects' muscle. Dytiscus and Hydrophilus muscle has received a large share of the attention of histologists, but that from the wasp or blue-bottle fly is quite as good. A leg should be pulled from the trunk of a blue-bottle fly and this again forcibly separated at the middle joint. A piece of muscle will project from one of the segments, which may be cut off and examined in a drop of fluid expressed from the thorax of the fly. The polariscopic effects may then be made clearly out in the still contracting fibres. I have

tested all these points by a careful examination of insects' fibres with thin plates of selenite and mica. This method is not so satisfactory, nor do the differences of colour seen give such trustworthy evidence as may be obtained by the crossed Nicols alone.

The Fibre during Contraction.—Living insects' muscle may be examined and the changes observed when the waves of contraction pass along the fibre, or perhaps better still, they may be fixed with osmic acid. The muscles from the leg of an insect are rapidly separated out on a slide, and a drop of weak osmic acid added which kills the fibres instantaneously, fixing them in the position that they happen to be in. On examination one generally finds fibres which in part of their course are contracted, and in other parts relaxed, when the differences in appearance may readily be studied. It may here be observed that the fibres bulge at the contracted part, so that if the surfaces be examined the focus of the microscope must be accommodated.

The cross stripes are nearer one to another and correspond, as before, with the ridges, and valleys seen at the margin, which are much more prominent and bolder in outline.

In the Contracted Fibre the Striping is practically the same as in the Stretched Condition.—The contracted fibre exhibits just the same reversing of stripes on alteration of focus, and Dobie's line and Hensen's stripe can both be seen in the same positions as in the uncontracted muscle, provided the fibre is suitably placed for examination and not sheared in its length. We must entirely deny the common statement, first introduced, we believe, by Merkel and Engelmann, that in the contracted state the bright band becomes the darker. If good specimens of insects' muscle be examined, which have been treated with osmic acid, and if the fibre be not sheared, the valley is always bright in the ordinary or deeper focus. I have verified this point in very many cases. Passing along a fibre from the relaxed end to a part where the contraction is fullest, the appearances vary in degree, but not in kind. The main features are in both cases the same, but the stripes are now narrower, and often it is not so easy to see Dobie's and Hensen's stripes. This follows from the statement of Engelmann, viz., that "the bright stripes become darker than the dim;" for he himself notices that at one point, or phase, in the contraction, no striping is to be made out. We agree with Ranvier that this is not true, indeed it would be impossible for a muscular fibre with its configuration not to be marked across its length.

This subject will call up to the mind of every working histologist, appearances which he must have met with in other fields of research. Many tissues naturally, or after clumsy manipulation, present ampullations which always co-exist with cross striæ. The fibres of the crystalline lens are wavy in outline, and when many of them are

bound together and seen on edge with the wavy outline towards the eye of the observer, cross bands are seen which in chance preparations (especially those of the frog's lens) simulate muscle in a wonderful manner. Ordinary non-striped muscle which may be so well seen in the frog's bladder is often faintly ampullated especially, perhaps, in chloride of gold preparations. Cross stripes may also here be seen. The fibres of Tomes, when a section of softened tooth is teazed, are pulled out of the dentinal tubules, and, being of a soft and somewhat elastic nature, on breaking they become often very beautifully ampullated, and it would be impossible to distinguish them from muscular fibrillæ. In the class of practical histology, on more than one occasion, students have asked me the meaning of beautiful cross shadings seen on nerve fibres; a slight ampullation which fully accounted for it, was always found.

Many more of such instances could be recalled in the experience of every one; it is needless to enumerate further.

In the winter of 1879–80, while examining fibres of the muscles of a newly-born child, a very curious discovery was made. A nucleus belonging to the sarcolemma was seen beautifully striped. It was not in close apposition to the fibre, a very narrow chink intervening. On focussing with great care, it was seen that the cross bands upon it corresponded with those of the adjoining fibre, a dark one, however, for a light one, and *vice versâ*. (Fig. 6.) Now, the curious point was that the nucleus had evidently been impressed by the fibre, moulded upon it, as it were, and on being pulled apart had presented a perfect cast of the surface. One would hardly believe in sarcous elements here. Last summer (1880) my friend Mr. Priestley communicated to me a similar and independent observation of his own, as a contribution towards the maintenance of my views upon the formation of the stripes.

The position that we have reached is this: a muscular fibre presents such cross markings, varying with shifting the lens up or down, as a filament of homogeneous structure and similar shape. I have shown this experimentally, and have illustrated it by simple experiments, which it is in the power of anyone to test. This being the case, I have searched to find if there be reason to assert any want of uniformity along the fibre, using various methods of staining. This I have failed to do, and have shown that the views commonly held are to be explained simply by the shapes of the fibres. As to the action of muscle on polarised light, I saw reason to dissent from the views of Brücke, and subsequently found my own in accordance with those of other recent observers. I differ from them in the explanation I offer of the two dark bands seen with crossed Nicols, for here, again, the shape of the fibre explains their presence without looking for any special structure.

So far we are led to consider the fibre as made up of many ampullated fibrils, packed side by side, forming an ampullated fibre, these fibrils being uniform throughout, and joined together by some cementing material, the nature of which we will not surmise. The only point which would suggest a definite structure along the fibril is the attachment of the sarcolemma in insects' muscle to Dobie's lines. There is no doubt that this membrane dips down and seems prolonged into Dobie's lines in a most beautiful and regular manner. The significance of this is very obscure, and is quite beyond me. There are many possibilities. It may be, although there is no proof of it, that a membrane exists here continuous with the sarcolemma; it may be that there is nothing but some cementing substance more soluble in alcohol than the sarcous matter: it may be that there is a little minor crest at this point to which the sarcolemma is attached. This little crest I have certainly seen in some fibres, and it has already been figured by more than one writer, yet in other fibres, the outlines of which are wonderfully distinct, no trace of it is to be made out. The fibres can hardly be said to break across in the line of Dobie, all that can definitely be affirmed is that they cleave in the thinnest part, or the light stripe. The investigation of this point is one of great difficulty, owing to the haze around the broken points, and I can never make up my mind to any definite statement. This transverse cleavage is not of course a point of very much weight, as the fibre would naturally tend to split across in or near Dobie's lines, as here it is thinnest.

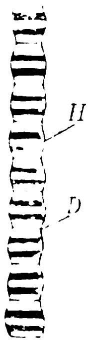
The striping of muscle can be easily explained, as shown before, which leads me to my final statement. A fibril is structureless throughout its entire length, except that, perhaps, there may be membranes, or lines of fission, or layers of cement at the positions of the lines of Dobie; this we leave an open question. In using the word "structureless," I must not be misunderstood; structureless membranes and tissues are fast losing their place in histology, and once simple protoplasm is now most complex. What I infer is that the stripes do not mark the positions of alternating layers of different structure, the presence of which are ordinarily maintained. The complicated "Muskelkästchen" of the Germans do not exist.

The muscular tissue of the heart presents many peculiarities which it is needless here to enumerate, for the cross striping alone concerns us. All those cross bands which have been described in ordinary voluntary muscle may here also be seen, and they are placed in the same relations with the turned surface of the fibre. The dark stripe corresponding to the crests, or ridges, the light bands to the depressions between them. (Fig. 7.) Dobie's lines may be made out with great ease, and as there is no sarcolemma here, they may be accounted for also purely from the shape of the fibres. I have often thought that Dobie's lines marked the positions of tiny ridges in the valleys, but this is a

point more difficult to decide perhaps than in the case of the skeletal muscles. Transverse cleavage takes place here also in the thinner part of the fibre, namely, in the bright stripe, but whether or not exactly in Dobie's line I have not yet definitely made out.

A curious appearance often presented by insects' muscle, and sometimes also by that of the mammalia, has been described and figured by Mr. Schäfer. A paper descriptive of these he communicated to the Royal Society of London (1873), which came out later on in the "Transactions" of this Society, and his observations are published also in the eighth edition of Quain's "Anatomy." These have been almost entirely overlooked by French and German physiologists, yet in many English laboratories his observations have been verified, and his conclusions taught.

They are well illustrated in a representation of the muscular fibre of a Dytiscus, which may be seen in Quain's "Anatomy." The dark stripes are traversed longitudinally by dark rods, which end at both extremities in little knobs. These knobs lie in the borderland between the bright and dark stripes. The only point which I would add to his figure is this, that the knobs are joined across the clear stripes or valleys by lines, just as they are so joined across the dark stripes, although the lens must be depressed ever so little to make this out. These lines are, in fact, nothing more or less than the longitudinal striæ described many years ago as lying between the fibrillæ of which the fibre is composed, these little knobs lying in their course. This can, perhaps, most conclusively be made out in the following way. Allow a piece of insect's muscle to remain in a drop of water for some hours (which will vary with the temperature) until it has partially putrefied. Then cover and examine, when many of the fibres will have separated towards their ends into fibrillæ. One can then distinctly trace the chinks between the separated fibrillæ as being continuous with the striæ, on which the knobs are still seen, in the centre of the fibre. I think that the following is a feasible explanation of these knob-like enlargements of the cementing substance seen as longitudinal striæ. These knobs occur, as will be beautifully seen on referring to the woodcut in Quain, on the slopes between the valleys and the ridges. The cementing substance dips down here with the fibre itself, and if there be the slightest lateral obliquity it will appear larger. The cementing matter is seen on edge, and differing as it does from the muscle-substance in refrangibility, a distortion occurs, giving rise not to a dark line as on the surface, but to a dark knob. This is, in fact, but an optical delusion, for the striæ are quite uniform, and were the fibre cylindrical would appear so. This may be proved by the fact that very often if the rays of light from the reflector are oblique, but one set of dots appears, which shift over to the other side on twisting the mirror. By shifting the preparation



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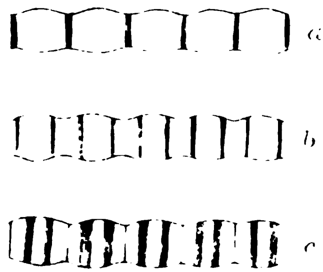

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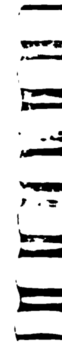

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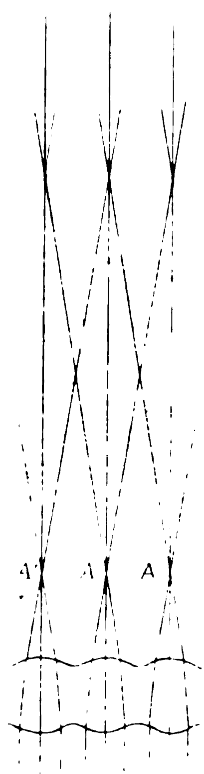

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about, or by twisting the tube of the microscope obliquely, the dots disappear from one part of the fibre to appear in another, showing that it is but an optical effect, and that no structure here exists.

Before concluding I must gratefully acknowledge much help and sympathy which I have received in this investigation.

To Professor Tait I have gone when in any difficulty, for an observer in a case such as this must have the aid of an experienced physicist, otherwise grievous error is but courted. To him, as has been seen in the text, I owe many suggestions, and he has kindly entirely looked over my paper. Dr. Klein has shown me great kindness in carefully examining my preparations from the histological point of view, and as has before been mentioned, in showing me preparations to corroborate my views. My thanks are also due to my friend Mr. John Priestley, for many hints, especially concerning the literature of the subject.

DESCRIPTION OF PLATE 5.

Figure 1. This represents a muscular fibre viewed with a very high power. The borders are wavy and the cross stripes correspond with these inequalities. (D) marks the positions of Dobie's dark stripes placed in the centres of the depression seen at the border. (H) represents Hensen's stripes, or Dobie's light stripes, placed on the summit of the ridges, in the centre of the dark band.

Figure 2 shows the appearance of the fibre with crossed Nicols. The shaded parts are seen on the slopes between the ridges and depressions. They are explained fully in the text.

Figure 3. A fibre is represented as seen with three positions of the lens. In (*a*) the lens is elevated and the depressions appear dark. In (*c*) the lens is fully depressed when the stripes are reversed, the depressions being now light with Dobie's dark stripe in the centre of them, and the crests dark but with Dobie's light stripe in the midst. In (*b*) an intermediate stage is seen.

Figure 4. This shows the passage of rays of light through the fibre. The convex parts converge the rays to focus A′, A″, A‴, after which they diverge. The lens shifted up or down (vertically) over the ridges or depressions, will focus on the retina alternately converging and diverging rays.

Figure 5. Muscular fibres of Echinus, described by Messrs. Geddes and Beddard.

Figure 6 represents a nucleus seen by me, which impressed on the muscle, is moulded to the same shape and appears to be cross striped.

Figure 7 shows the striping of the muscular tissue of the heart.

II. "Description of some Remains of the Gigantic Land-lizard (*Megalania prisca*, OWEN) from Australia. Part III." By Professor OWEN, C.B., F.R.S., &c. Received January 20, 1881.

(Abstract.)

In this communication the author describes additional parts of the *Megalania*, reconstructed from fossils exhumed by Mr. George Fred. Bennett, at the same locality as the subjects of Part II, and subsequently transmitted. They were found about thirty feet distant from the spot where the cranial fossils were imbedded, and are deemed by their discoverer to be parts of the skeleton of the same individual.

The recognisable restorations constitute the termination and a detached annular segment of the bony sheath, with one inclosed vertebra, of the tail. The average thickness of the sheath's substance is one inch; the coalesced portion includes three segments; and, save the last, these with the antecedent detached segment support osseous conical processes, in structure resembling the horn-cores of the cranium, but of larger dimensions. Each segment supports two pairs of such solid cones or cores. The transverse diameter of the antepenultimate segment taken across the tips of a pair of cores is eleven inches; the same diameter of the area of such annular sheath is five inches; the vertical diameter of the exterior of the sheath is five inches and a-half. These dimensions support Mr. Bennett's conclusion as to their relation to the skull; and, supposing the lizard's body originally entombed not to have undergone a dislocation affecting the two extremities now brought to light, their relative distance agrees with the length of the animal, estimated from proportions of previously described vertebræ.

Detailed descriptions, with figures, of the parts of the horn-bearing tail-armour are given, and comparisons are subsequently pursued in examples of recent and fossil *Reptilia* and *Mammalia*, provided with similar caudal armature.

Illustrations of these and of the parts of *Megalania* compared accompany the paper.

III. "On a Method of Destroying the Effects of slight Errors of Adjustment in Experiments of Changes of Refrangibility due to Relative Motions in the Line of Sight." By E. J. STONE, F.R.S., Director of the Radcliffe Observatory, Oxford. Received January 17, 1881.

Let arrangements be made for the reversion of the prisms without any disturbance of the other optical arrangements, including, of course, the position of the cylindrical lens, if one be used. Any slight errors of adjustment which prevent the light from the star and the comparison light from falling upon the train of prisms under the same optical circumstances, so far as mere direction is concerned, will have opposite effects in the reversed positions of the prisms; but the separation of the emergent lights due to relative motion will remain unchanged by the reversal of the positions of the prisms.

If, therefore, the apparent change of refrangibility due to relative motion remains unchanged by the reversion of the prisms, all doubts about the effects of errors of adjustment will be removed. But if the results in the reversed positions of the prisms sensibly differ, then the existing errors of adjustment must be removed, or their effects allowed for by taking a mean of the results in reversed positions, before any reliance can be fairly placed upon the determination of relative motions in the line of sight.

A reversible spectroscope was arranged by me, and made by Mr. Simms, some years ago, but I have never since had an equatoreal, with a good driving clock, under my control with which the experiment indicated could be properly tried.

With the direct prisms now in use, the required reversion can be easily arranged. I am not likely, for some time, to have the use of a good equatoreal, and I, therefore, publish the plan with the hope that some one more fortunately situated may give it a fair trial.

The experiment is a crucial one, and, in my opinion, should be tried.

IV. "On an Improved Bimodular Method of computing Natural and Tabular Logarithms and Anti-Logarithms to Twelve or Sixteen Places, with very brief Tables." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received January 17, 1881.

SECTION I.—NATURE OF THE BIMODULAR METHOD AND ITS IMPROVEMENT.

The *Bimodulus* is a constant, which is exactly double of the modulus of any system of logarithms. The *Bimodular Method* is derived from

the familiar proposition that, when the difference of two numbers is small, the difference of their logarithms is nearly equal to the bimodulus multiplied by the difference and divided by the sum of the numbers themselves. The improvement here for the first time effected, consists in prefixing a brief *preparation*, which makes the method universally applicable, and subjoining an easy *correction* depending on the transformation of a well-known series proceeding by the odd powers of the difference divided by the sum of two numbers, whereby the number of places obtained is greatly increased. This method is here applied for finding the natural and tabular logarithms of any number to twelve places of figures by means of a table of two pages for each kind of logarithm, and to sixteen places by help of a seven-place table of tabular or Briggs's logarithms. An extremely simple rule, which, so far as I know, was never before imagined, enables us to pass from the logarithm to the number, that is, to find anti-logarithms from the same tables. Although the method is applicable to any system of logarithms, and was actually first applied by me to the direct calculation of musical logarithms to the bases 2 (octave), $\sqrt[12]{2}$ (equal semitone), and $81 \div 80$ (comma), and appropriate tables have been constructed, I confine myself for brevity to natural and tabular logarithms. The tables are constructed from existing materials, but the method is capable of constructing them independently.

Section II.—Principles of the Bimodular Method and its Improvement.

Fundamental Relations.—Let n and d be any whole numbers of which d is the smaller, and let $p=d \div n$, a proper fraction. Let

$$\text{nat. log}\,(1+p)=y, \text{ and } \log\,(1+p)=\text{M}y \quad . \quad . \quad . \quad (1)$$

where M is the modulus, and hence 2M the bimodulus to any unspecified system of logarithms marked by log. Let

$$\frac{d}{2n+d}=\frac{p}{2+p}=q,\ 2q=x,\ 2\text{M}q=\text{M}x=z \quad . \quad . \quad . \quad . \quad (2).$$

In future n and $n+d$ will often be called "the numbers," n "the tabular number," d "the difference," 2Md "the dividend," $2n+d$ "the sum" or "divisor," and $2\text{M}d \div (2n+d)$ "the quotient."

Now it is familiarly known that

$$y=p-\tfrac{1}{2}p^2+\tfrac{1}{3}p^3-\tfrac{1}{4}p^4+ \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

$$=2(q+\tfrac{1}{3}q^3+\tfrac{1}{5}q^5+\ldots) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Putting in (4) the values of q in terms of x and z from (2) we have

$$y=x+\tfrac{1}{12}x^3+\tfrac{1}{80}x^5+\tfrac{1}{448}x^7+\ldots=x+c \quad . \quad . \quad . \quad . \quad (5),$$

$$My=z+\tfrac{1}{12}\cdot\frac{z^3}{M^2}+\tfrac{1}{80}\cdot\frac{z^5}{M^4}+\tfrac{1}{448}\cdot\frac{z^7}{M^6}+\ldots=z+Mc \quad (6).$$

And putting for x and z their values from (2) we find

$$2p=(2+p)x,\ 2Mp=(2+p)z,\ \text{whence}\ 1+p=\frac{2+x}{2-x}=\frac{2M+z}{2M-z}. \quad (7),$$

And by expanding the first of these equations (7)

$$x=p-\tfrac{1}{2}p^2+\tfrac{1}{4}p^3-\tfrac{1}{8}p^4+\ldots \quad . \quad . \quad . \quad . \quad . \quad (8).$$

Subtracting (8) from (3), and multiplying by M to find the Mc of (6), we have

$$My-z=Mc=M(\tfrac{1}{12}p^3-\tfrac{1}{8}p^4+\tfrac{11}{80}p^5-\tfrac{13}{96}p^6+\tfrac{57}{448}p^7-\ldots) \quad . \quad . \quad (9),$$

a converging series of which the limits are the first term and the first two terms.

Preparation.—To insure p being small in all cases, I have invented the rule of *preparation*, founded on the fact that if N be the number whose logarithm is sought, and a and b any two numbers of which the logarithms are known, such that $Na\div b=n+d$, where n is the next less number to $Na\div b$ in the table, and d, the difference, is less than the difference between two numbers in the table, then $\log N=\log(n+d)+\log b-\log a$. In Tables I and II the difference between two consecutive numbers is ·001, and as there are 100 entries, all the numbers lie between 1 and 1·1; so that if $Na\div b$ is less than 1·1, the required reduction is effected.

Preparation is accomplished in two lines of simple multiplication and division, as follows:—

The given number N is divided or multiplied by such a power of 10 as will leave the quotient or product as a decimal fraction between 1 and 10. This is effected by simply shifting the decimal point.

If the first decimal place is less than 3 times the integer (which is always the case when the integer exceeds 3), divide by the integer and divide the quotient by 1·1 or 1·2. The result is less than 1·1.

If the first decimal place is more than twice the integer, then it is always possible, generally in several ways, to find an integer between 1 and 10 which, used as a multiplier, will give a product of which the integer is less than 13, and the first decimal place less than the integer. The following rule embraces every case:—Multiply any of the numbers 1·30 to 1·340 by 4; 1·340 to 1·80 by 7; 1·80 to 1·960 by 5; 1·960 to 1·99 by 6; 2·50 to 2·99 by 4; 3·80 to 3·99 by 3. Then dividing this product by the integer the quotient is less than 1·1.

This preparation is very convenient also for starting Weddle's and

Hearn's processes given by Mr. Peter Gray in the introduction to his Tables for twelve-place logarithms, 1865 (first published in 1845), and is also very much simpler than that proposed by Mons. Thoman in his "Tables de logarithmes à 27 décimales," 1867.

Interpolation.—The finding of log N is thus made dependent on finding log $(n+d)$, where n is a tabular number and d is less than ·001. We then find $2\text{M}d \div (2n+d)$, which gives the "uncorrected" logarithm of $n+d$, or the "quotient" x or z. The multiplication $2\text{M} \times d$ is effected by the multiples of the bimodulus given in the tables, when M is not 1, the unit place of each multiple of 2M being placed immediately below the determining figure of d, care being taken to preserve as many places as are necessary for the final result. The division is a single contracted division. The resulting x or z has to be "corrected" by the equations (5) and (6), as shown in Section III.

Completion.—Having found log $(n+d)$, we add the logarithm of the power of 10 by which we first divided, and the logarithm of any other divisor, and the arithmetical complement of the logarithm of the power of 10 or any other multiplier. All these logarithms are given in the table. The result is the complete log N to the number of decimal places for which the table is adapted.

Anti-Logarithms.—A logarithm being given we have to reduce it to the logarithm of a number between 1 and 1·1. This is most conveniently done by subtracting from it (or adding to it) the logarithm of the largest power of 10, which will make the result lie between 0 and log 10, and afterwards subtracting the next least logarithm of an integer between 1 and 10, and then the next least logarithm of a number between 1·1 and 2. The logarithms of all these numbers are given in the table. The result will be the logarithm of a number less than 1·1. We then subtract the next less logarithm in the table of interpolation, and obtain the equivalent to the *corrected* quotient $x+c$ or $z+\text{M}c$ of (5) and (6). We find the correction in the same way as for the quotient, and *subtract* it, thus obtaining x or z. Then we divide the bimodulus increased by this x or z, by the bimodulus decreased by this x or z, as in (7), and thus find $1+p$, which is the number corresponding to the "quotient" in the direct method. For "completion" this has to be multiplied by the numbers corresponding to all the logarithms subtracted in the preparation.

Section III.—Calculation of the Bimodular Corrections.

The principal peculiarity of this improved bimodular method consists in the calculation of the corrections and the determination of the number of places which can be trusted in any case, as assigned in the tables.

The repetition of any digit m times within the same number will

be represented by suffixing m to the right of that digit. Thus, $\cdot 0_m 1$ is a decimal fraction beginning with m zeroes and followed by 1, and nat. log $1\cdot 0_7 1 = \cdot 0_8 9_8 5 0_7 3_8 0 8 3_6 5 3_7$ to forty-eight places. Other writers have used 0^m in this sense, but it is not applicable to other digits, and conflicts with the usual notation of powers, thus 230^3 looks like $(230)^3 = 12,167,000$, in place of $23,000 = 230_3$.

Write equations (5) and (6) thus—

$$\text{nat. log}\,(1+p) = x + c = x + c_1 + c_2 + \dots \qquad (10),$$

$$\text{tab. log}\,(1+p) = z + t = z + t_1 + t_2 + \dots \qquad (11),$$

where

$$c_1 = \tfrac{1}{12} x^3 = x^3 \times \cdot 0833 \dots, \quad c_2 = \tfrac{1}{80} x^5 = x^5 \times \cdot 0125 \qquad (12),$$

$$\text{tab. log}\, c_1 = 3\,\text{tab. log}\, x + \cdot 920\ 8188 - 2 \qquad (13),$$

$$\text{tab. log}\, c_2 = 5\,\text{tab. log}\, x + \cdot 096\ 9100 - 2 \qquad (14),$$

$$\text{tab. log}\, x = \tfrac{1}{3}\,\text{tab. log}\, c_1 + \cdot 359\ 7271 \qquad (15),$$

$$t_1 = z^3 \times \cdot 441\ 824\ 842\ 539\ 87, \quad t_2 = z^5 \times \cdot 351\ 376\ 544\ 673\ 68. \qquad (16),$$

$$\text{tab. log}\, t_1 = 3\,\text{tab. log}\, z + \cdot 645\ 2501 - 1 \qquad (17),$$

$$\text{tab. log}\, t_2 = 5\,\text{tab. log}\, z + \cdot 545\ 7728 - 1 \qquad (18),$$

$$\text{tab. log}\, z = \tfrac{1}{3}\,\text{tab. log}\, t_1 + \cdot 118\ 2500 \qquad (19).$$

By means of these equations the corrections can be calculated from the "quotient" (that is, the approximate values of x and z) either with or without existing tables of logarithms, or the quotient x or z may be calculated to which a particular value of the first correction is due.

From these has been calculated the following table of the critical values of the first and second corrections, upon which the whole practical use of the corrections depends. The quotients were first taken to proceed from $\cdot 0_m 1$ to $\cdot 0_m 9$ by steps of $\cdot 0_m 1$. Then the values of the quotients were determined, which reduced either of the two first corrections to $\cdot 0_n 1$, n being variable, from which point the suffix of 0, or the number of initial zeroes, changed, giving critical values of the corrections. Such quotients were then inserted in numerical order. The approximate numbers were obtained from the quotients on the supposition that p was small enough to make nat. log $(1+p) = p$, and tab. log $(1+p) = Mp$, to three places of significant figures.

The suffix of 0 in the first correction, diminished by 1, shows the number of places which are unaffected by that correction, that is, the number of places in the uncorrected quotient which may be trusted without corrections. The undiminished suffix shows a number of places which cannot be wrong by more than one unit in defect in the

Table of the Critical Values of the Corrections.

I. Natural logarithms.

Approximate number.	Exact quotient.	Value of c_1.	Value of c_2.
$1 \cdot 0_m 100$	$\cdot 0_m 100$	$\cdot 0_{3m+4} 833$	$\cdot 0_{5m+6} 125$
106	106	$\cdot 0_{3m+3} 100$	169
152	152	290	$\cdot 0_{5m+5} 100$
200	200	666	400
229	229	$\cdot 0_{5m+2} 100$	786
240	240	116	$\cdot 0_{5m+4} 100$
300	300	225	304
381	381	460	$\cdot 0_{5m+3} 100$
400	400	533	128
493	493	$\cdot 0_{3m+1} 100$	365
500	500	104	391
600	600	180	972
604	604	183	$\cdot 0_{5m+2} 100$
700	700	285	210
800	800	426	409
900	900	607	738

II. Tabular logarithms.

Approximate number.	Exact quotient.	Value of t_1.	Value of t_2.
$1 \cdot 0_m 281$	$\cdot 0_m 100$	$\cdot 0_{3m+3} 442$	$\cdot 0_{5m+5} 352$
284	123	827	$\cdot 0_{5m+4} 100$
303	131	$\cdot 0_{3m+2} 100$	137
450	195	329	$\cdot 0_{5m+3} 100$
462	200	353	112
653	282	$\cdot 0_{5m+1} 100$	636
693	300	119	654
713	310	165	$\cdot 0_{5m+2} 100$
925	400	283	360
$1 \cdot 0_{m-1} 100$	$\cdot 0_m 434$	$\cdot 0_{3m+1} 361$	$\cdot 0_{5m+2} 542$
113	491	522	$\cdot 0_{5m+1} 100$
116	500	552	110
139	600	954	273
141	609	$\cdot 0_{3m} 100$	295
163	700	152	590
179	778	208	$\cdot 0_{5m} 100$
186	800	226	115
209	900	322	207

last place. The suffix of 0 in the second correction, diminished by 1, shows how many places of the quotient, after applying the first correction, are left unaffected by the second correction, that is, how many places can be trusted on applying the first correction only. For natural logarithms it will be seen that this never gives less than $5m+1$ places, that is, $2m+1$ places in addition to those determined without correction. Thus in Table I, where m is never less than 3, we can always obtain sixteen places. For tabular logarithms, as in Table II, we must first observe a critical value in the numbers themselves. In that table the number $1+p$, whose logarithm is finally sought, must be less than 1·001. Hence, while in the upper part of the preceding table of critical values, m will always be 3 or more, in the lower part, $m-1$ will always be 3 or more, so that m will always be 4 or more. As far then as the quotient $\cdot 0_3 434$, the first correction gives only $5 . 3+1=16$ places, and this is the largest quotient that can commence with $\cdot 0_3$. If the significant figures are greater than 434, then m will be 4, and up to the quotient $\cdot 0_4 778$ we can trust $5 . 4=20$ places, and beyond it we can even trust 19 places. Observe that $\cdot 0_4 9$ at the bottom of this table is followed by $\cdot 0_3 1$ at the top (II, second column), for which, also, the second corrections leave $5 . 3+4=19$ places unaffected.

But in determining the full number of places of the first correction from the uncorrected quotient by equations (12) and (16), we are, of course, obliged to take so many significant places, that on cubing the result and multiplying by the proper coefficient, no error affecting the full number of places should be committed. The number of places required for this purpose is so large that if we calculated the result directly, the present method of correction would be illusory. Hence it is necessary to use common seven-place logarithmic tables which can be trusted to six places. Consequently, we can use only six significant places in the quotient for finding the correction, and we thus introduce an error not exceeding half a unit in the last place in excess or defect. On estimating the limiting effect of this error, I find practically that on using six significant places of the uncorrected quotient to determine the first correction, we may trust all six places of the correction found. The total number of places that can be trusted, when this error is allowed for, depends on the quotient. Let r be the significant places of the quotient converted into a decimal fraction with one unit place. Then the real quotient is $\cdot 0_m 1 \times r$, but on taking only six significant places, we use as a quotient $\cdot 0_m 1 \times r \pm \cdot 0_{m+6} 1 \times 5$, and the error thus made in the correction may be taken as the term involving r^2 in the cube of this number divided by $12M^2$, that is, as $\cdot 0_{3m+8} 1 \times 15r^2 \div 12M^2$. Then, putting $15r^2 \div 12M^2 = 10$ and 100, and finding the corresponding values of r, we obtain the critical values of the quotient where the suffix of 0 in the error of the

Bimodular Table I.—Natural Logarithms.

1. Table for Interpolation.

No.	Natural Logarithm.	No.	Natural Logarithm.
1·000	·000 000 000 000 000 000	1·050	·048 790 164 169 432 003
1	·000 999 500 333 083 533	51	·049 742 091 894 814 074
2	·001 998 002 662 673 056	52	·050 693 114 315 518 118
3	·002 995 508 979 798 478	53	·051 643 233 151 838 450
4	·003 992 021 269 537 453	54	·052 592 450 119 170 584
1·005	·004 987 541 511 039 074	1·055	·053 540 766 928 029 818
6	·005 982 071 677 547 464	56	·054 488 185 284 069 731
7	·006 975 613 736 425 242	57	·055 434 706 888 100 582
8	·007 968 169 649 176 874	58	·056 380 333 436 107 639
9	·008 959 741 371 471 904	59	·057 325 066 619 269 407
1·010	·009 950 330 853 168 083	1·060	·058 268 908 123 975 776
11	·010 939 940 038 334 364	61	·059 211 859 631 846 083
12	·011 928 570 865 273 802	62	·060 153 922 819 747 091
13	·012 916 225 266 546 328	63	·061 095 099 359 810 876
14	·013 902 905 168 991 421	64	·062 035 390 919 452 641
1·015	·014 888 612 493 750 655	1·065	·062 974 799 161 388 435
16	·015 873 349 156 290 149	66	·063 913 325 743 652 797
17	·016 857 117 066 422 899	67	·064 850 972 319 616 314
18	·017 839 918 128 331 000	68	·065 787 740 538 003 097
19	·018 821 754 240 587 761	69	·066 723 632 042 908 173
1·020	·019 802 627 296 179 713	1·070	·067 658 648 473 814 805
21	·020 782 539 182 528 504	71	·068 592 791 465 611 716
22	·021 761 491 781 512 692	72	·069 526 062 648 610 245
23	·022 739 486 969 489 429	73	·070 458 463 648 561 419
24	·023 716 526 617 316 042	74	·071 389 996 086 672 945
1·025	·024 692 612 590 371 501	1·075	·072 320 661 579 626 121
26	·025 667 746 748 577 792	76	·073 250 461 739 592 673
27	·026 641 930 946 421 178	77	·074 179 398 174 251 512
28	·027 615 167 032 973 365	78	·075 107 472 486 805 412
29	·028 587 456 851 912 555	79	·076 034 686 275 997 608
1·030	·029 558 802 241 544 403	1·080	·076 961 041 136 128 325
31	·030 529 205 034 822 873	81	·077 886 538 657 071 225
32	·031 498 667 059 370 991	82	·078 811 180 424 289 778
33	·032 467 190 137 501 495	83	·079 734 968 018 853 559
34	·033 434 776 086 237 388	84	·080 657 903 017 454 467
1·035	·034 401 426 717 332 396	1·085	·081 579 986 992 422 874
36	·035 367 143 837 291 316	86	·082 501 221 511 743 696
37	·036 331 929 247 390 277	87	·083 421 608 139 072 391
38	·037 295 784 743 696 896	88	·084 341 148 433 750 885
39	·038 258 712 117 090 341	89	·085 259 843 950 823 419
1·040	·039 220 713 153 281 296	1·090	·086 177 696 241 052 332
41	·040 181 789 632 831 832	91	·087 094 706 850 933 767
42	·041 141 943 331 175 177	92	·088 010 877 322 718 299
43	·042 101 176 018 635 394	93	·088 926 209 194 401 509
44	·043 059 489 460 446 977	94	·089 840 703 999 789 463
1·045	·044 016 885 416 774 327	1·095	·090 754 363 268 464 143
46	·044 973 365 642 731 158	96	·091 667 188 525 823 792
47	·045 928 931 888 399 803	97	·092 579 181 293 093 194
48	·046 883 585 898 850 420	98	·093 490 343 087 338 889
49	·047 837 329 414 160 123	99	·094 360 675 421 484 311

2. ***For Preparation.***

No.	Natural Logarithm.					
1·1	0·095	310	179	804	324	869
1·2	0·182	321	556	793	954	626
1·3	0·262	364	264	467	491	052
1·4	0·336	472	236	621	212	931
1·5	0·405	465	108	108	164	382
1·6	0·470	003	629	245	735	554
1·7	0·530	628	251	062	170	396
1·8	0·587	786	664	902	119	008
1·9	0·641	853	886	172	394	776
2·0	0·693	147	180	559	945	309
3·0	1·098	612	288	668	109	691
4·0	1·386	294	361	119	890	619
5·0	1·609	437	912	434	100	375
6·0	1·791	759	469	228	055	001
7·0	1·945	910	149	055	313	305
8·0	2·079	441	541	679	835	928
9·0	2·197	224	577	336	219	383
10·0	2·302	585	092	994	045	684
11·0	2·397	895	272	798	370	544
12·0	2·484	906	649	788	000	310

3. ***Multiples of nat. log* 10.**

No. of mult.	Natural Logarithm.					
1	2·302	585	092	994	045	684
2	4·605	170	185	988	091	368
3	6·907	755	278	982	137	052
4	9·210	340	371	976	182	736
5	11·512	925	464	970	228	420
6	13·815	510	557	964	274	104
7	16·118	095	650	958	319	788
8	18·420	680	743	952	365	472
9	20·723	265	836	946	411	156
10	23·025	850	929	940	456	840
11	25·328	436	022	934	502	524
12	27·631	021	115	928	548	208
13	29·933	606	208	922	593	892
14	32·236	191	301	916	639	576
15	34·538	776	394	910	685	260
16	36·841	361	487	904	730	944

4. ***For no Corrections.***

For Difference, or Quotient,	Trust places, uncorrected,	
$\cdot 0_2 100$	9	And one place
$\cdot 0_3 493$	10	more in each case
$\cdot 0_3 229$	11	with a probable
$\cdot 0_3 106$	12	error in it of one
$\cdot 0_4 493$	13	unit in defect.
$\cdot 0_4 229$	14	
$\cdot 0_4 106$	15	
$\cdot 0_5 193$	16	

For intermediate quotients trust the number of places opposite the next greater in the above table.

5. ***For Full Corrections, Additive.***

Take six significant figures of the quotient, and use six significant figures of the cor. from this formula—

tab. log cor. = 3 tab. log quotient + ·920 8188 − 2.

Trust all the places thus corrected, that is—

For Difference, or Quotient,	Trust in result places,	
$\cdot 0_2 100$	14	And one place more in each
$\cdot 0_3 894$	15	case with a probable error in it
$\cdot 0_3 284$	16	of one unit in defect.
$\cdot 0_3 100$	17	
$\cdot 0_4 894$	18	

For intermediate quotients trust the number of places opposite the next greater.

6. ***For Short Corrections, Additive, giving twelve places.***

Work to thirteen places. Possible error on "completion" one unit in the twelfth place. For intermediate quotients use the correction opposite the next less.

Quotnt.	Cor.	Quotnt.	Cor.	Quotnt.	Cor.
$\cdot 0_3 000$	$\cdot 0_{10} 00$	$\cdot 0_3 707$	$\cdot 0_{10} 30$	$\cdot 0_3 894$	$\cdot 0_{10} 60$
182	1	715	31	899	61
262	2	723	32	904	62
311	3	731	33	909	63
348	4	738	34	913	64
$\cdot 0_3 378$	$\cdot 0_{10} 05$	$\cdot 0_3 745$	$\cdot 0_{10} 35$	$\cdot 0_3 918$	$\cdot 0_{10} 65$
404	6	752	36	923	66
427	7	759	37	928	67
448	8	766	38	932	68
467	9	773	39	937	69
$\cdot 0_3 485$	$\cdot 0_{10} 10$	$\cdot 0_3 780$	$\cdot 0_{10} 40$	$\cdot 0_3 941$	$\cdot 0_{10} 70$
501	11	786	41	946	71
517	12	792	42	950	72
531	13	798	43	952	73
545	14	805	44	959	74
$\cdot 0_3 558$	$\cdot 0_{10} 15$	$\cdot 0_3 811$	$\cdot 0_{10} 45$	$\cdot 0_3 963$	$\cdot 0_{10} 75$
571	16	817	46	968	76
583	17	823	47	972	77
594	18	829	48	976	78
606	19	835	49	980	79
$\cdot 0_3 616$	$\cdot 0_{10} 20$	$\cdot 0_3 841$	$\cdot 0_{10} 50$	$\cdot 0_3 984$	$\cdot 0_{10} 80$
627	21	846	51	989	81
637	22	852	52	993	82
646	23	857	53	997	83
656	24	863	54		
$\cdot 0_3 665$	$\cdot 0_{10} 25$	$\cdot 0_3 868$	$\cdot 0_{10} 55$		
674	26	873	56		
683	27	879	57		
691	28	884	58		
699	29	889	59		

Bimodular Table II.—Tabular Logarithms.

1. Table for Interpolation.

No.	Tabular Logarithm.	No.	Tabular Logarithm.
1·000	·000 000 000 000 000 000	1·050	·021 189 299 069 938 073
1	·000 434 077 479 318 641	51	·021 602 716 028 242 220
2	·000 867 721 531 226 912	52	·022 015 739 817 720 259
3	·001 300 933 020 418 119	53	·022 428 371 185 486 518
4	·001 733 712 809 000 530	54	·022 840 610 876 527 803
1·005	·002 166 061 756 507 676	1·055	·023 252 459 633 711 470
6	·002 597 980 719 908 592	56	·023 663 918 197 793 454
7	·003 029 470 553 618 007	57	·024 074 987 307 426 268
8	·003 460 532 109 506 486	58	·024 485 667 699 166 953
9	·003 891 166 236 910 522	59	·024 895 960 107 485 003
1·010	·004 321 373 782 642 574	1·060	·025 305 865 264 770 241
11	·004 751 155 591 001 063	61	·025 715 383 901 340 666
12	·005 180 512 503 780 310	62	·026 124 516 745 450 260
13	·005 609 445 360 280 428	63	·026 533 264 523 296 757
14	·006 037 954 997 317 171	64	·026 941 627 959 029 378
1·015	·006 466 042 249 231 723	1·065	·027 349 607 774 756 528
16	·006 893 707 947 900 450	66	·027 757 204 690 553 459
17	·007 320 952 922 744 597	67	·028 164 419 424 469 893
18	·007 747 778 000 739 942	68	·028 571 252 692 537 612
19	·008 174 184 006 426 395	69	·028 977 705 208 778 017
1·020	·008 600 171 761 917 561	1·070	·029 383 777 685 209 641
21	·009 025 742 086 910 247	71	·029 789 470 831 855 634
22	·009 450 895 798 693 927	72	·030 194 785 356 751 215
23	·009 875 633 712 160 158	73	·030 599 721 965 951 084
24	·010 299 956 639 811 952	74	·031 004 281 363 536 802
1·025	·010 723 865 391 773 104	1·075	·031 408 464 251 624 136
26	·011 147 360 775 797 468	76	·031 812 271 330 370 371
27	·011 570 443 597 278 197	77	·032 215 703 297 981 585
28	·011 993 114 659 256 928	78	·032 618 760 850 719 897
29	·012 415 374 762 432 929	79	·033 021 444 682 910 673
1·030	·012 837 224 705 172 205	1·080	·033 423 755 486 949 702
31	·013 258 665 283 516 547	81	·033 825 693 953 310 343
32	·013 679 697 291 192 549	82	·034 227 260 770 550 632
33	·014 100 321 519 620 579	83	·034 628 456 625 320 360
34	·014 520 538 757 923 700	84	·035 029 282 202 368 120
1·035	·014 940 349 792 936 558	1·085	·035 429 738 184 548 315
36	·015 359 755 409 214 218	86	·035 829 825 252 828 143
37	·015 778 756 389 040 962	87	·036 229 544 086 294 540
38	·016 197 353 512 439 047	88	·036 628 895 362 161 100
39	·016 615 547 557 177 412	89	·037 027 879 755 774 956
1·040	·017 033 339 298 780 355	1·090	·037 426 497 940 623 635
41	·017 450 729 510 536 156	91	·037 824 750 588 341 878
42	·017 867 718 963 505 669	92	·038 222 638 368 718 428
43	·018 284 308 426 530 869	93	·038 620 161 949 702 792
44	·018 700 498 666 243 352	94	·039 017 321 997 411 969
1·045	·019 116 290 447 072 807	1·095	·039 414 119 176 137 143
46	·019 531 684 531 255 434	96	·039 810 554 148 350 354
47	·019 946 681 678 842 334	97	·040 206 627 574 711 132
48	·020 361 282 647 707 846	98	·040 602 340 114 073 104
49	·020 775 488 193 557 860	99	·040 997 692 423 490 567

Bimodular Table II.—Tabular Logarithms—*continued.*

2. *For Preparation.*

No.	Tabular Logarithm.
1·1	0·041 392 685 158 225 041
1·2	0·079 181 246 047 624 828
1·3	0·113 943 352 306 836 769
1·4	0·146 128 035 678 238 026
1·5	0·176 091 259 055 681 242
1·6	0·204 119 982 655 924 781
1·7	0·230 448 921 378 273 929
1·8	0·255 272 505 103 306 070
1·9	0·278 753 600 952 828 962
2·0	0·301 029 995 663 981 195
3·0	0·477 121 254 719 662 437
4·0	0·602 059 991 327 962 390
5·0	0·698 970 004 336 018 805
6·0	0·778 151 250 383 643 633
7·0	0·845 098 040 014 256 831
8·0	0·903 089 986 991 943 586
9·0	0·954 242 509 439 324 875
10 0	1·000 000 000 000 000 000
11·0	1·041 392 685 158 225 041
12·0	1·079 181 246 047 624 827

3. *Multiples of the Bimodulus.*

No. of mult.	Value of Multiple.
1	0·868 588 963 806 503 655
2	1·737 177 927 613 007 311
3	2·605 766 891 419 510 966
4	3·474 355 855 226 014 621
5	4·342 944 819 032 518 277
6	5·211 533 782 839 021 932
7	6·080 122 746 645 525 587
8	6·948 711 710 452 029 242
9	7·817 300 674 258 532 898

4. *For no Corrections.*

Difference.	Quotnt.	Trust Places	
$\cdot 0_3 100$	$\cdot 0_3 431$	9	And one more place in each case with a probable error in it of one unit in defect.
$\cdot 0_3 653$	$\cdot 0_3 282$	10	
$\cdot 0_3 303$	$\cdot 0_3 131$	11	
$\cdot 0_3 141$	$\cdot 0_4 609$	12	
$\cdot 0_4 653$	$\cdot 0_4 282$	13	
$\cdot 0_4 303$	$\cdot 0_4 131$	14	
$\cdot 0_4 141$	$\cdot 0_5 609$	15	
$\cdot 0_5 653$	$\cdot 0_5 282$	16	

For intermediate Differences and Quotients trust the number of places opposite the next greater.

5. *For Full Corrections, Additive.*

Take six significant figures of the quotient and use six significant figures of the correction from this formula—

tab. log cor. = 3 tab. log quotient + ·645 2501 − 1.

Difference.	Quotnt.	Trust places	
$\cdot 0_3 100$	$\cdot 0_3 434$	14	And one more place in each case with a probable error in it of one unit in defect.
$\cdot 0_3 893$	$\cdot 0_3 388$	15	
$\cdot 0_3 284$	$\cdot 0_3 123$	16	
$\cdot 0_3 231$	$\cdot 0_3 100$	17	
$\cdot 0_4 893$	$\cdot 0_4 388$	18	

For intermediate Differences and Quotients trust the number of places opposite the next greater.

6. *For Short Corrections, Additive, giving twelve places, with a possible error of one unit in the twelfth place on completion.*

Quotient.	Correction.	Quotient.	Correction.
$\cdot 0_3 000$	$\cdot 0_{10} 00$	$\cdot 0_3 353$	$\cdot 0_{10} 20$
104	01	359	21
150	02	365	22
178	03	371	23
199	04	376	24
$\cdot 0_3 217$	$\cdot 0_{10} 05$	$\cdot 0_3 381$	$\cdot 0_{10} 25$
232	06	386	26
245	07	391	27
257	08	396	28
268	09	401	29
$\cdot 0_3 278$	$\cdot 0_{10} 10$	$\cdot 0_3 406$	$\cdot 0_{10} 30$
288	11	410	31
296	12	415	32
305	13	419	33
313	14	423	34
$\cdot 0_3 320$	$\cdot 0_{10} 15$	$\cdot 0_3 427$	$\cdot 0_{10} 35$
327	16	432	36
334	17		
341	18		
348	19		

For intermediate quotients take the correction opposite the next less.

Note.—The natural logarithms to eighteen places in Table I are either taken direct or calculated (by subtracting nat. logs of 1,000 and 10) from "Wolframii Tabula Logarithmorum Naturalium" to forty-eight places, appended to Vega's "Thesaurus Logarithmorum Completus," 1794.

The tabular logarithms to eighteen places in Table II are taken direct from Mr. Peter Gray's "Tables for the formation of Logarithms and Anti-Logarithms to twenty-four places," 1876.

In both tables the arrangement and corrections are original.

correction changes. The results are given under "5. Full Corrections," in Tables I and II.

But although it is by no means difficult or very troublesome to use the formulæ (13) and (17) for finding the first correction, it is always inconvenient to use two tables. It would be manifestly impossible to give a table of corrections to six figures within reasonable limits. Hence, leaving the "full correction" to be found, when desired, by these formulæ, I append a table of "short corrections," so as to obtain twelve places of the result from Tables I and II at sight. The thirteenth place has been allowed for, so that the result may be thoroughly trusted, but in the "completion" an error of one unit in the twelfth place may easily creep in unless "full corrections" are used. These "short corrections" have been calculated from the formulæ (15) and (19), by assuming successive values of the first correction, as $\cdot 0_{12}5$, $\cdot 0_{11}15$, $\cdot 0_{11}25$ and so on, and calculating the corresponding value of the quotients. But in the table itself these corrections are entered as $\cdot 0_{11}1$, $\cdot 0_{11}2$, &c. The limiting correction is reached when the corresponding quotient is the next least to that due to the number 1·001. These twelve places are fully as many as are required for ordinary purposes, and for them only thirteen out of the eighteen places in the tables should be used.

Section IV.—Bimodular Tables and Examples.

Table I applies to natural logarithms giving from nine to sixteen places, according to circumstances, with no corrections, twelve places with short corrections, and fourteen to sixteen places with full corrections.

Rule to find the logarithm from the number.—Reduce the given number to the form of a decimal fraction with an integer less than 10.

Multiply and divide by such whole numbers less than 13 as will reduce the number to one less than 1·1, as shown in Section II.

Find the next less number in "1. Table for Interpolation," and first subtract it from the reduced number, then omit the decimal point, and multiply by 2, forming the "dividend;" secondly, add this next less number to the reduced number, and then omit the decimal point, forming the "divisor."

Divide the dividend by the divisor by simple contracted division to as many places as are required. Correct the quotient, as may be necessary, by the table or formula of correction, No. 6 or 5.

Add the logarithms of the divisors and the arithmetical complements of the logarithms of the multipliers used in forming the reduced number, to find the full corrected logarithm.

Table II applies to Briggs's or Tabular Logarithms, giving from nine to sixteen places, according to circumstances, with no correc-

tions, twelve places with short corrections, and fourteen to eighteen places with full corrections.

Rule.—Proceed precisely as for natural logarithms, except that instead of multiplying by 2 it is necessary to multiply by the tabular bimodulus, by help of the multiples given in No. 3.

Tables I and II. *Rule to find the number from the logarithm.*—Subtract the logarithm of the next lower power of 10, and then, in order, the next lower logarithm in the lower, and then that in the upper part of the table "2. For Preparation," and afterwards the next lower logarithm in the table for interpolation.

Considering this as an approximate logarithm of a reduced number, find the correction as if it were a quotient by No. 5 or 6, and *subtract* (instead of adding) the correction, which reduces it to the form of a quotient or approximate logarithm.

Add the resulting number to and subtract it from the bimodulus (which is 2 for natural logarithms) and divide the sum by the difference.

Multiply the quotient in succession by the numbers corresponding to the logarithms subtracted. The result is the number required.

Examples, fully worked out, with explanations.

Let $$N=192\ 699\ 928\ 576=(76)^6.$$

Then calculating the value of 6 nat. log 76 from Wolfram's tables appended to Vega's, and multiplying the result by the tabular modulus we find to twenty places—

$$\text{nat. log } N=25{\cdot}984\ 400\ 041\ 717\ 986\ 473\ 06$$

$$\text{tab. log } N=11{\cdot}284\ 881\ 553\ 684\ 748\ 111\ 78$$

These numbers serve as checks to the correctness of the following work.

Here a, b, c form the "preparation" of N. As a begins with 1·9, where the first decimal place is more than 3 times the integer, a is multiplied by 6 to produce 11·56 . . ., a decimal fraction of which the integer 11 is less than 13 and more than twice the first integer 5. Both 5 and 4 would have also answered. The divisor 11 is separated off by), and in the quotient c the next less number 1·051 in the table for interpolation is similarly separated. This leaves c—1·051 to the right of), with the decimal point already omitted. Then this difference is multiplied by the bimodulus 2, to obtain the dividend d. The whole of c is added to the separated part 1·051, and then the decimal point is omitted, giving e. As the difference $c={\cdot}0_4905$, lies between ${\cdot}0_3106$ and ${\cdot}0_4493$, we can certainly obtain twelve places without correction (Table I, No. 4), and as it lies between ${\cdot}0_31$ and ${\cdot}0_4894$ we can obtain seventeen places with full corrections (Table I, No. 5). We

Ex. 1. To Table I.—Find nat. log N to sixteen places. The letters refer to the following explanations. Every figure required by the most moderate calculator is inserted.

	$a=N\div10^{11}$.	1·92 699 928 576		*a*
	$b=6a$.	11) ·56 199 571 456		*b*
	$c=b\div11$.	1·05 1)09 051 950 545 454 54		*c*
e	210 209 051 950 545 454 54	)18 103 901 090 909 09		*d*
	. ...		16 816 724 156 043 63	(8
f	·000 086 123 318 301 099		1 287 176 934 865 46	
g	$\cdot0_{13}$ 53 233		1 261 254 311 703 27	(6
h	·049 742 091 894 814 074		25 922 623 162 69	
k	2·397 895 272 798 370 544		21 020 905 195 05	(1
l	8·208 240 530 771 944 999−10		4 901 717 967 14	
m	25·328 436 022 934 502 524		4 204 181 039 01	(2
n	25·984 400 041 717 986 473		697 536 928 13	
			630 627 155 90	(3
	$d=2(c-1\cdot051)\times10^{19}$=dividend.		66 909 772 23	
	$e=2(c+1\cdot051)\times10^{19}$=divisor.		63 062 715 59	(3
	$f=d\div e$=quotient.		3 847 056 64	
	g=full correction, see below.		2 102 090 64	(2
	h=nat. log 1·051.		1 744 966 12	
	k=nat. log 11.		1 681 672 41	(8
	l=arithm. comp. of nat. log 6.		63 293 71	
	m=11 nat. log 10.		63 062 72	(3
	n=nat. log N, true to 18 places.		230 99	
			210 21	(01
	Calculation of g. Log f, taking six significant places,		20 78	
	=log $\cdot0_4861$ 233= ·935 1206 − 5		18 92	(09
	3 log f=2·805 3618 −15		1 86	
	+ ·920 8188 − 2		1 89	(9
	log g=log $\cdot0_{13}532$ 329= ·726 1806 −14			

prepare, then, for seventeen places, by carrying the quotient *c* far enough to allow of obtaining eighteen places, that is, fourteen significant places of the quotient *f*. As at least 2 digits of the divisor must remain for the last contracted divisor, we shall require only fifteen places of the divisor, and the last five are rejected (shown by drawing a line under them). The successive digits of the quotient are written to the right after ((following Briggs's use), and are collected in *f*. The rest of the process is evident from the notes

made. The result happens to be correct to eighteen places, in place of the guaranteed seventeen; but this is quite accidental, as the last or eighteenth place of all the logarithms used is always in excess or defect.

Ex. 2. To Table II.—Find tab. log N to twelve places by the short corrections.

$a=N\div10^{11}$.	1·92 699 928 576		a
$b=5a$.	9) ·63 499 642 880		b
$c=b\div9$.	1·07 0)55 515 875 556		c
$d=5\times$ bimodulus $\times10^{10}$.		43 429 448 190	d
5× ,, $\times10^{9}$.		4 342 944 819	
5× ,, $\times10^{8}$.		434 294 482	
$e=1\times$,, $\times10^{7}$.		08 685 890	e
5× ,, $\times10^{6}$.		4 342 945	
8× ,, $\times10^{5}$.		694 871	
7× ,, $\times10^{4}$.		60 801	
5× ,, $\times10^{3}$.		4 343	
5× ,, $\times10^{2}$.		434	
5× ,, $\times10$.		43	
6× ,,		5	
g	214 055 515 875 556)	48 220 476 823	f
	· ··· ··· ··	42 811 103 175	(2
h	·000 225 270 891 2	5 409 373 648	
k	·0_{11} 5	4 281 110 318	(2
m	·029 383 777 685 2	1 128 263 330	
n	·954 242 509 439 3	1 070 277 579	(5
p	·301 029 995 664 0−1	57 985 751	
q	11·0	42 811 103	(2
r	11·284 881 553 684 7	15 174 648	
		14 983 886	(7
$f=(c-1\cdot070)\times10^{14}\times$ bimodulus = dividend.		190 762	
$g=(c+1\cdot070)\times10^{14}$ = divisor.		171 244	(08
$h=f\div g$ = quotient.		19 518	
k = short correction for quotient ·000 225.		19 265	(9
m = tab. log 1·070.		253	
n = tab. log 9.		214	(1
p = arithm. comp. of tab. log 5.		39	
q = 11 tab. log 10.		40	(2
r = tab. log N, correct to 13 places.			

Ex. 3. To Table II. Given the tab. log N, to eighteen places of decimals, to find N to the greatest possible number of digits. This process is entirely new, and depends upon Section II, eq. (7).

a=tab. log N.	11·284 881 553 684 748 112	a
b=tab. log 10^{11}+tab. log 1·9.	11·278 753 600 952 828 962	b
$c=a-b$.	·006 127 952 731 919 150	c
d=tab. log 1·014.	·006 037 954 997 317 171	d
tab. log $e=c-d$.	·000 089 997 734 601 979	tab. log e
f=correction, see below.	·0_{12} 322 066	f
tab. log e'=tab. log $e-f$.	·000 089 997 734 279 913	tab. log e'
h=bimodulus.	·868 588 963 806 503 655	h

l	·868 498 966 072 223 742)	·868 678 961 540 783 568	k
	... ...	868 498 966 072 223 742	(1
e	1·000 207 248 915 1(8(7 4	179 995 468 559 826	
m	10 002 072 489 151 9	173 699 793 214 445	(0002
n	4 000 828 995 660 7	6 295 675 345 381	
p	1·014 210 150 400 000 (0	6 079 492 762 505	(07
q	·912 789 135 360 000 0	216 182 582 876	
N	1926 999 285 76·0 000 0	173 699 793 214	(2
		42 482 789 662	
	$k=h$+tab. log e'.	34 739 958 643	(4
	$l=h-$tab. log e'.	7 742 831 019	
	$e=k\div l$.	6 947 991 728	(8
	$m=e\times$·01.	794 839 291	
	$n=e\times$·004.	781 649 069	(9
	$p=e+m+n=e\times 1$·014.	13 190 222	
	$q=p\times$·09.	8 684 990	(1
	$\mathrm{N}=(p+q)\cdot 10^{11}=p\times 1{\cdot}09\times 10^{11}$.	4 505 232	
		4 342 495	(5
	Calculation of f—	162 737	
	r=tab. log e= ·$0_4$899 977 taken as quotient in	86 850	(1
	s=tab. log r= ·954 2314− 5 Table II, No. 5.	75 887	
	$3s$=2·862 6942−15	69 480	(8
	t= ·645 2501− 1	6 407	
	$3s+t$=log f= ·507 9443−13	6 079	(7
		328	
		260	(3
		68	

The preparation a, b, c, is similar to that in Ex. 1, but 5 is used as the multiplier by way of variety. The difference $c-1{\cdot}070$ being $\cdot 0_3 55$ . . ., which lies between $\cdot 0_3 653$ and $\cdot 0_3 303$, we cannot be certain of more than ten places without correction (Table II, No. 4). As only twelve places are wanted, we use the short corrections and work to thirteen places. The chief peculiarity relates to the multiplication of $c-1{\cdot}070$ by the bimodulus by means of the multiples in Table II, No. 3, omitting all the decimal points. The integer of the multiple is placed under the determining figure of the multiplicand, and the multiple is then written out as far as necessary, neglecting the point, but regulating the last figure. It is best to write in the integer 0, as in line e, to preclude error. As the quotient must begin with $\cdot 0_3$, only ten significant places are wanted, and hence only eleven places in the divisor g, the four underlined 5556 are therefore rejected. The correction k is found from Table II, No. 6, as belonging to a quotient between $\cdot 0_3 217$ and $0_3 232$. The rest is sufficiently explained in the notes. The result is accidentally correct to thirteen places.

Here a is the given tab. log to eighteen places. We first subtract 11 log 10, or the characteristic. Next, if the remainder were greater than any logarithm in the lower part of Table II, No. 2, we should subtract that. But in this case it is not, and hence we proceed to the upper part of No. 2, and subtract the next less, or tab. log 1·9. This completes the preparation, as the difference $c=a-b$, lies between the tab. logs of 1·014 and 1·015 in No. 1, the table for interpolation. Hence, subtracting tab. log 1·014, we find tab. log e, of which the number e has to be found. Now, the formula (7) applies only to an uncorrected $z=$tab. log e', which cannot differ from tab. log e in the three first significant figures. In the direct process, tab. log e is found from tab. log e' by adding the correction found by Table II, No. 5. Hence we have only to subtract this correction f, which is calculated from the same first six significant figures in both cases, as shown in the example. Having found this uncorrected tab. log e', we add it to and subtract it from, the bimodulus, obtaining k and l respectively, and thus find $e=k\div l$. Now, tab. log e' cannot be greater than the greatest difference between two tab. logs in Table II, No. 1, "for interpolation," that is, it cannot be greater than ·000 434 077 . . ., and hence than $\cdot 001 \times$ modulus. Hence the result of this division $k\div l$, must be less in any case than $(2\text{M}+\cdot 001\times\text{M})\div(2\text{M}-\cdot 001\times\text{M})=$ 1·0010005 . . . , and must be greater than 1, hence it must commence with 1·000. As the modulus divides out, this conclusion holds for all systems of logarithms. As the last divisor in the contracted division must have two digits for safety, it follows that the number of digits in the quotient $k\div l=e$, will be one less than the number of digits in the divisor, that is, than the number of decimal places in the given logarithm. And as the first of these digits is a whole number,

it follows that the number of decimal places in the quotient $k+l$, will be two less than in the given logarithm. Moreover, as the last decimal place is always approximate, it follows that the number e cannot be found with certainty to more than three decimal places less than the number of decimal places in the given tab. logarithm. Hence, in the present case, although tab. log e is known to eighteen places of decimals, e is known with certainty only to fifteen places of decimals (and sixteen digits). But the error in the next place (or digit) will not probably exceed one unit.

Having found e, we have to multiply it in succession by the numbers corresponding to the logarithms subtracted in the preparations in this example, 1·014, 1·9, and 10^{11}. This is most readily done in the way sufficiently explained by the notes in the example. The resulting number is accidentally correct to seventeen digits, but only sixteen can be used with certainty. Hence, if we use this bimodular method of finding logarithms and anti-logarithms, we should always find the logarithms to two or three places of decimals more than we require digits in the final number to be found.

V. "On the Potential Radix as a Means of Calculating Logarithms to any Required Number of Decimal Places, with a Summary of all Preceding Methods Chronologically Arranged." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received January 17, 1881.

In the tables attached to my paper "On an Improved Bimodular Method of Computing Logarithms, &c." ("Proc. Roy. Soc.," vol. 31, p. 381), the logarithms used were all taken direct, or immediately calculated, from the tables of Wolframm and Gray. But a complete method of calculating logarithms should be independent of extraneous aid and be applicable to the first construction of tables of logarithms. I shall here show that my improved bimodular method is capable of furnishing a practical means of calculating natural logarithms, and hence logarithms to any base and to any number of places of decimals.

By the term *positive numerical radix* I shall understand a table of the numbers r, $1{\cdot}r$, $1+{\cdot}0_m r$, with their corresponding natural logarithms, where r varies from 1 to 9, 0_m means a series of m zeroes, and m varies from 1 to any required number. The word *Radix* in this sense is adopted from R. Flower, 1771, mentioned below. By the term *negative numerical radix* I mean a similar table of $1-{\cdot}0_m r$, and the negatives of their corresponding logarithms. When these radixes (forming an English plural, as *radices* would be misleading) have been

constructed to the requisite number of decimal places, logarithms and anti-logarithms can be calculated by various methods. The improved bimodular method requires a much less extended radix for the same number of decimal places than any other method. Thus, to find tab. log N=6 tab. log 76=tab. log 192 699 928 576, the example used in my former paper. The requisite radix logarithms are assumed from Gray and Thoman. The details of division and multiplication are omitted for brevity.

First with the positive numerical radix,

1·92 699 928 576	$a=\mathrm{N}\div10^{11}$
9) ·63 499 642 880	$b=5a$
1·07)055 515 875 555 555 555 556	$c=b\div9$
1·000 5)18 839 958 463 136 033	$d=c\div1{\cdot}07$
16 364 179 999 652 896	$e=2\mathrm{M}(d-1{\cdot}0005)$
2·001 0 18 839 958 463 136 033	$f=d+1{\cdot}0005$
0·000 0 08 177 924 001 951 217	$g=e\div f$
$\cdot0_{15}$ 241 645	h=correction
·000 217 092 972 230 208 282	=tab. log 1·0005
·029 383 777 685 209 640 835	=tab. log 1·07
·301 029 995 663 981 195 214	−1=compt. tab. log 5
·954 242 509 439 324 874 590	=tab. log 9
11·0	=tab. log 10^{11}
11·284 881 553 684 748 111 783	sum=tab. log N

For the details of the method see my former paper. The preparation is here carried a step further than there, before the interpolation. We first multiply by 5 and then divide by 9, as in Ex. 2 of my former paper, but the division by 9 is now carried to twenty-three places of decimals, and then 1·07, a number in the radix, is separated off as a new divisor of c, giving d, where 1·0005 is separated off as the next less number in the radix. But we might have used it as a divisor, and should have then found 1·00 001 833 078 307 160 0 . . . from which still more decimal places could be obtained in the final result. But stopping at d, we form the dividend e and divisor f, and then find the quotient g. The correction is obtained from the formula in my former paper, Table II, No. 5. But as these corrections involve the use of other tables, they would be illegitimate in first constructions, which would give only fourteen decimal places correct in place of twenty. The rest is as usual.

The disadvantage of this method by the positive numerical radix, is

the necessity for frequent divisions, as by 1·07, 1·0005, 1·00001, &c., very simple, it is true, but rather lengthy. These are avoided by the use of the negative numerical radix.

Taking the preparation a, b, c as before, we begin with c, and instead of dividing out by 1·07, we only see how often 1·07 will go in the three first significant figures 705, and finding it to be 6, we multiply c by 0·6, producing d, and subtracting this from c, we obtain $d=1-\cdot 06$. After this the number of times that 1006 goes in 6321 is 6, the first significant four places of decimals. Hence we multiply e by $\cdot 0_2 6$, subtract the result f from e, and obtain $e\times(1-\cdot 0_2 6)$. It is evident that by this process the number of zeroes with which the decimal fraction commences can be increased by at least one at every multiplication by $(1-\cdot 0_m r)$ a number in the negative radix. We stop when sufficient zeroes are obtained to apply the improved bimodular method for a sufficient number of places. We might, however, have continued the process till the number of zeroes were half of the decimal places, and then the "divisor" would be practically 2, and hence the rule of multiplying the difference (which would be the remaining significant figures) by the bimodulus and dividing by the

1·07 055 515 875 555 555 555 5(55	c
6 423 330 952 533 333 333 333	$d=c\times\cdot 06$
1·00 632 184 923 022 222 222(222	$e=c-d=c\times(1-\cdot 06)$
603 793 109 538 133 333 333	$f=e\times\cdot 0_2 6$
1·00 028 391 813 484 088 88(8 888	$g=e-f=e\times(1-\cdot 0_2 6)$
20 005 678 362 696 817 777	$h=g\times\cdot 0_3 2$
1·00 008)386 135 121 392 071 111	$k=g-h=g\times(1-\cdot 0_3 2)$
335 392 704 979 237 550	$l=2M(k-1\cdot 0_4 8)$
2·00 016 386 135 121 392 071 111	$m=k+1\cdot 0_4 8$
. ...	
·000 001 676 826 141 397 547 6	$=l\div m$
$\cdot 0_{17}$ 2 083 1	correction
·954 242 509 439 324 874 590 1	tab. log 9
·301 029 995 663 981 195 213 7−1	comp. tab. log 5
·026 872 146 400 301 340 372 0	$-$tab. log $(1-\cdot 06)$
·002 613 615 602 686 687 981 2	$-$tab. log $(1-\cdot 0_2 6)$
·000 086 867 583 428 580 794 6	$-$tab. log $(1-\cdot 0_3 2)$
·000 034 742 168 884 033 200 5	tab. log $1\cdot 0_4 8$
11·0	tab. log 10^{11}
11·284 881 553 684 748 111 782 8	sum=tab. log N

sum (in this case 2) would amount to multiplying the significant figures by the modulus only, which is Thoman's rule. In that case there would be no correction. The completion consisting of adding the logarithms of the divisors and subtracting those of the multipliers, is the usual one, but as the negative radix gives —tab. log $(1-{\cdot}0_m r)$ direct, there is no occasion for using arithmetical complements. We might, also, have continued the process till all the decimal places were zero, and then have made the whole work one of completion. The last half of the process need not be gone through, as the multipliers in the negative radix can be taken from it at sight. This is the process of Mr. Weddle, the inventor of the negative radix.

The number may be recovered from the logarithms in various ways from the positive radix, and, among others, by my improved bimodular method, or in Mr. Weddle's method, from the negative radix. Hence the problem is reduced to finding a simple way of calculating the positive and negative radixes. Before explaining the method proposed in this paper, however, it will be best to prefix a chronological summary of the methods actually proposed for calculating logarithms with or without a radix, and with or without an annexed indication of the means employed for calculating the radix.*

Chronological Summary of Methods.

1624. **Briggs*, H. "Arithmetica Logarithmica," p. 32, contains the first positive numerical radix, under the name of § "Tabella inventiõni Logarithmorum inserviens," giving r, $1{\cdot}r$, $1{\cdot}0_m r$ from $r=1$ to $r=9$, and $m=1$ to $m=8$ with their tabular logarithms to fifteen places of decimals. The fifteenth place is often more than one unit wrong, and two other errors occur, namely, tab. log $4={\cdot}60205\ 9991$ . . . . for ${\cdot}60205\ 9990$ . . ., corrected in the chiliads, and tab. log $1{\cdot}0_3 5={\cdot}0_3 21\ 700$ . . . for ${\cdot}0_3 21\ 709$ . . ., which last error is reproduced in the chiliads. Briggs does not explain how he calculated this table. He uses it to interpolate in his chiliads, and finds the logarithm by means of a series of divisions with a continually augmented divisor, which is in fact the product of the successive factors into which the number is gradually resolved, but he does not explain this contrivance. He finds the number corresponding to the logarithm by subtracting the next less logarithms successively and multiplying by the corresponding numbers, a method generally adopted.

* The works marked * are in the library of the Royal Society, the originals of those marked †, and transcripts of the whole of the necessary portions of those marked §, were given to the Royal Society by the author of this paper when it was read, so as to put a tolerably complete collection of all the papers bearing upon the subject in the possession of the Royal Society.

1628. **Vlacq*, A. In his second edition of Briggs, 1628, and not in those printed later, gives Briggs's §Radix to ten places only, but repeats the two internal errors mentioned above, correcting both of them, however, in his chiliads. There is no notice of this radix in *Vega's edition of Vlacq, under the name of "Thesaurus Logarithmorum," 1794.

1714. **Long*, J. "Philosophical Transactions," vol. 29, 1717, No. 339, pp. 52—54. A radix of *logarithms* of the form $\cdot r$, $\cdot 0_m r$ from $r=1$ to $r=9$, and $m=1$ to $m=7$, and their corresponding natural numbers, intermediates being found by continual divisions. He finds the numbers by "one extraction of the fifth or sursolid root for each class," and for a method of performing that extraction refers to Halley's paper on finding the roots of equations, in *"Phil. Trans," vol. 18, for 1694, pp. 136—148.

1742. †*Gardiner*, W. Tables of Logarithms. He gives a table of logarithms to twenty places of decimals for the numbers 1 to 1143, 101000 to 101139, and 00000 to 00139 (the two last with the first, second, and third differences), and a rule whence, by the help of these tables, the logarithm to any number is found to twenty places of decimals. No explanations.

1771. §*Flower*, Robert. "The Radix, a new way of making Logarithms." Flower introduces the word *Radix*, here preserved *in memoriam*. He apparently used it because he considered all numbers between 1 and 10 to be *roots* of 10. He applies the term to several tables. First, "the cube radix of 10," a series of cube roots, 10, $\sqrt[3]{10}$, $\sqrt[3]{\sqrt[3]{10}}$, &c., each expressed as decimals to ten places, ending at $1\cdot 0_9 2=r$, and then each term is again expressed as a series of cubes of r. He shows how to find the tabular logarithms of any number from this table, and actually finds tab. log 2 to ten places of decimals in two different ways. He next calculates "the square Radix of 10," a series of square roots, 10, $\sqrt{10}$, $\sqrt{\sqrt{10}}$, &c., with indices of the powers of the last, $1\cdot 0_9 2=r$. By this he proved the work with the cube radix. But finding the labour much lessened by the smaller intervals between these square roots, and still more so when the two radixes were combined, he was led (p. 9) to the "classical radix," which is so called because of the "classes" into which the numbers $1\cdot 0_m r$ were divided by the different values of m, corresponding to my positive numerical radix, the number of the class being $m+1$. This he calculated, apparently from the two first radixes separately, or "both ways," as he says, to ten decimal places, to $1\cdot 0_9 1$. He subsequently enlarged his "square radix," under the name of the square-square radix, and added another, called the cube-square radix, of the form 10, $\sqrt[3]{10}$, $\sqrt{\sqrt[3]{10}}$, $\sqrt{\sqrt{\sqrt[3]{10}}}$, &c., and from these he calculated his classical radix up to $1\cdot 0_{11} 1$ and twenty-three places of decimals, of which he believed

twenty-two to be correct, as was actually the case, except for tab. logs of 1·4, $1{\cdot}0_4 5$, and $1{\cdot}0_8 9$, for which only twenty-one places were correct. To use this radix he gave three rules, all original, called the "direct," the "reverse," and the "reflected" rules. It is the last one which is most valuable, and which he mainly exemplifies. This rule consists in preparing the number by reducing it to a decimal fraction having 0 as its whole number, and then multiplying it in succession by numbers of the form $1{\cdot}0_m r$ till the result is unity, then the sum of the complements of the logarithms of these numbers (given in the radix) will be the logarithm of the reduced number. This was at the time an entirely original conception, and the method of working it out, which was totally different from Briggs's, gave the simplest means for finding tabular logarithms to twenty places. I give these details because Raper and Horsley, as well as Hutton (who reports their opinions, in the **first* edition only, 1785, of his mathematical tables, p. 72, foot note), who had evidently very insufficiently studied Flower's work, considered his process to be merely "a large exemplification" of Briggs's. Although Flower's method of finding the number from the logarithm agrees with Briggs, and although he speaks of Vlacq, I believe that he never saw either Briggs's or Vlacq's works containing the radix, which were expensive and difficult to procure. He seems to have known of them chiefly from *Sherwin's tables. Robert Flower was an obscure writing-master at Bishop's Stortford, where he was buried, aged 63 years and unmarried, on the 23rd of February, 1774, just three years after his book, "printed for the author," was published. It consequently rapidly disappeared. It is not mentioned in Mr. Glaisher's catalogue (Rep. Br. As., 1873), it is not in De Morgan's catalogue; I found no copy at the British Museum, at Oxford, or Cambridge, or at the Royal Society. But there were two copies in Mr. Graves's collection at University College, London, one of which, at my suggestion, has been presented to the British Museum.

1802. *Leonelli*, Z. "Supplément Logarithmique," Bordeaux, An. XI (1802–3). Leonelli re-discovered Briggs's method, and having fortunately obtained a copy of Flower's book from M. Evêque, who bought it in London, reproduced his radix for tabular logarithms to $1{\cdot}0_{10}1$ and up to twenty places only, added another radix for natural logarithms to the same extent, and gave Flower's rule, with his name. This work was translated into German by *Leonhardi in 1806, with numerous changes. Only one copy of the original work was known to exist, presented by the author to the city library of Bordeaux, from which it was †reprinted, with a preface, in 1876, by M. Hoüel, who had already given from it an account of Flower's rule, with a radix, name, and date, in his †"Tables de Logarithmes à Cinq Décimales," Table V, where he styles it "la méthode la plus simple de toutes celles

qui ont été proposées pour le même objet." From this work an account of Flower's method was introduced into an appendix to Don Vicente Vazquez Queipo's "Tablas de los Logarithmos Vulgares," from the †French edition of which I first heard of Flower's rule. Queipo added a twenty-first place from Thoman. Schrön gives Flower's radix to sixteen places. tabular and natural, with the rule, in his "Interpolations-Tafel," 1861, p. 76, probably from Léonhardi, but does not mention Flower's name, and the same omission occurs in †Hoüel's translation of the same.

1806. *§*Manning*, Thomas. "New Method of Computing Logarithms," "Phil. Trans.," 1806, p. 327. Manning was evidently unacquainted with Briggs, Flower, and Leonelli. His table is essentially a *potential negative radix* for natural logarithms, and as such was partly an anticipation of my conception, explained below. But he did not form the powers of $1-{\cdot}0_m1$, he merely tabulated $-r \log (1-{\cdot}0_m1)$ from $r=1$ to $r=9$ and $m=1$ to $m=8$, conceived only as $r \log \frac{10^m}{9^m}$. He therefore performed the division by the values of the powers of $1-{\cdot}0_m1$, by means of a continual multiplication by ${\cdot}0_m1$ and subtraction, which makes his process simple, but very lengthy. It is, however, entirely original. He did not apply his method to the discovery of the number from the logarithm.

1845. *§*Weddle*. "Computation of Logarithms and Anti-Logarithms," in "The Mathematician," November, 1845, pp. 17–25. He says his method was discovered in 1838, and gives it as a modification of Manning's. But it consists, in fact, of a complete *negative numerical radix* for both tabular and natural logarithms for sixteen decimal places down to $-\log (1-{\cdot}0_6 1)$, calculated by the usual series for $-\log (1-x)$, and applied, not only to finding the logarithms to numbers, but to finding numbers from logarithms. It is, therefore, really an original method, completely worked out, and the most important since Flower's. Extended tables were given by Shortrede, 1849.

1846. §*Gray*, Peter. "A Practical Method of Forming Logarithms and Anti-Logarithms," 8th December, 1846, reprinted from the "Mechanics' Magazine" for October and November, 1846, contains a re-arrangement of Weddle's plan, with improved tables.

1847. *§*Hearn*, Professor. "Practical Method of Forming Logarithms and Anti-Logarithms, independently of extensive Tables," in "The Mathematician" for March, 1847, pp. 249—252. This was an independent discovery of Weddle's method for finding logarithms by the negative nnmerical radix, but for finding numbers from logarithms he used the positive numerical radix. He gives tables to ten places of decimals, down to $-$tab. $\log (1-{\cdot}0_9 1)$, but does not mention how they were calculated. Extended tables were given by Shortrede, 1849.

1848. §*Gray*, Peter. "A Table for the Easy Formation of Anti-Logarithms, with its Application to the Converse Problem of the Formation of Logarithms," in the "Mechanics' Magazine" for the 12th and 26th February, 1848. This was founded on Hearn's paper, whence Mr. Gray obtained his first knowledge of a positive numerical radix, never having seen Briggs's or Flower's, and it contained the first of his enlarged positive numerical radixes for twelve decimal places, containing log r from $r=1$ to $r=9$, log $1\cdot r$, and log $1\cdot 0_{2m}r$, from $m=1$ to $m=5$, and $r=01$ to $r=99$. This he used only as an anti-logarithm process, proposing, for the discovery of logarithms, a continually augmenting divisor, which was, like Leonelli's, an independent discovery of Briggs's method, proceeding, however, by periods of two places instead of one.

1848. §*Orchard*, W., in the "Mechanics' Magazine" for 26th February, 1848, referring to Hearn's positive arithmetical radix, showed how it might be applied to finding the logarithm by a process amounting in fact to an independent re-discovery of Flower's reflected rule, using, however, Mr. Gray's tables of the 12th February, 1848, just mentioned. He also suggested another method derived from Manning's, by using factors of the form $1+\cdot 0_m 1$, which would amount to an anticipation of my potential positive radix described below, but it was differently conceived, and was worked out by the binomial theorem.

1849. †*Byrne*, Oliver. "Practical, Short, and Direct Method of Calculating the Logarithm of any Given Number, and the Number corresponding to any Given Logarithm," London (Appleton), 1849. This is an independent method. Mr. Byrne finds ten numbers between 1 and 10^{10}, the tab. logarithms of which, including the index, contain the same digits as the numbers themselves, to sixteen digits (except one which holds only for fourteen digits). Then, taking these as constants, he multiplies any number up to one of these numbers, by successive powers of $1\cdot 0_m 1$, using binomial coefficients, and subtracts the tab. logs of these powers from the tab. log of the constant. He finds the number from the logarithm by a similar process.

1851. †*Koralek*, Philippe. "Méthode Nouvelle pour calculer les Logarithmes des Nombres," Paris. This is a bimodular method, depending upon series (4) in my former paper (suprâ, p. 392). By a series of multipliers, he reduces all numbers to others lying between 800 and 1000, for which the first term of the series gives him seven places accurately, without corrections. He then calculates the succeeding terms of the series by a somewhat laborious process, and finds logarithms to twenty-seven places. His process differs entirely from mine, except in being originally bimodular.

1865. †*Steinhäuser*, A. "Kurze Hilfstafel zur bequemen Rechnung fünfzehnstelliger Logarithmen zu gegebenen Zahlen, und umgekehrt,"

Vienna. This is an extended positive numerical radix of the form r and $1+{\cdot}0_{3m}r$, where r varies from 001 to 999, and m from 1 to 2. The rule is one of continual division by the three, six, and nine first places respectively, similar to the first example in this paper, and, after nine figures are obtained, by a table of proportional parts.

1865. *Gray*, Peter. "Tables for the formation of Logarithms and Anti-Logarithms to twelve places, with explanatory introduction." This is an abridged anticipation of Mr. Gray's great tables to twenty-four places, calculated in 1856, and not published till 1876. It consists of an extended positive numerical radix for tabular logarithms, consisting of the tab. logs and complements of tab. logs of 1 to 9, and of the tab. logs of $1{\cdot}r$, $1+{\cdot}0_{3m}r$, from $r=001$ to $r=999$ and $m=1$ to $m=3$. The process is the same as in the paper of 1848, but with periods of three digits.

1867. **Thoman*, Fédor. "Tables de Logarithmes à 27 Décimales pour les Calculs de précision," Paris. These consist essentially of a positive and negative numerical radix, the first used for finding anti-logarithms by a process resembling Flower's and Hearn's, and the second to find logarithms by a process resembling Weddle's. His principal novelty consists in his table for preparation. After the result is reduced to the form $1{\cdot}0_m r$, where r consists of m digits, Thoman completes by adding $M\times{\cdot}0_m r$, for which he gives a special table. The positive radix extends to $1{\cdot}0_{13}1$, and the negative to $1-{\cdot}0_{13}1$, both calculated to twenty-seven places by an undescribed process. He makes no references to former writers; but one of his examples makes it probable that he knew Gray, 1865.

1871. *Pineto*, S. "Tables de Logarithmes vulgaires à 10 Décimales, construites d'après un nouveau mode, approuvées par l'Académie Impériale des Sciences de S. Pétersbourg," St. Petersburgh. An auxiliary table gives opposite the first four figures (or next least first four figures) of a number, a multiplier of at most three digits (with the complement of its logarithm, which will reduce the number to one between 1000 and 1010, and for all such reduced numbers tables are given, by which their logarithms can be readily found to ten places. The process of finding both logarithms and anti-logarithms by these tables (extending only to 56 pages octavo), is much simpler than by Vega's Thesaurius. But no new process of calculating logarithms originally is involved.

1873. †*Wace*, Rev. Henry. "On the Calculation of Logarithm" in the "Messenger of Mathematics," New Series, No. 29, 1873. The tables consist of a positive and negative numerical radix to $1\pm{\cdot}0_{10}1$, and to twenty places of decimals for both tabular and natural logarithms. The tabular were taken partly from Shortrede, and read with Callet, and partly from H. M. Parkhurst's astronomical calculations. The natural logarithms were calculated independently, with a few

exceptions, which were taken from Callet. The process is essentially the same as Weddle's and Hearn's, but was discovered independently.

1876. **Gray*, Peter. "Tables, &c., to Twenty-four Places, with explanatory introduction and historical preface." See above, 1865. The tables are an extended positive numerical radix, containing $1\cdot r$, $1\cdot 0_{3m}r$ from $r=001$ to $r=999$, and $m=1$ to $m=4$, and by inference to $m=7$, and twenty-four places of decimals. The tables were calculated to twenty-seven places, and verified to twenty-four by laborious processes fully described, but as far as possible, Abraham Sharpe's and Wolframm's tables were employed. The process is that of 1848, adapted to periods of three digits. Mr. Thomas Warner, who assisted Mr. Gray to publish these tables, showed how they might be applied to Flower's rule in periods of three figures by means of *Crelle's *Rechentafeln* for the multiplication by three digits. This is the latest simplification of Flower's rule. I have been much indebted to Mr. Gray's historical preface, and to the loans of papers and books from him in the compilation of this list, but I have personally examined every process I have described.

1876. †*Hoppe*, Professor Dr. Reinhold. "Tafeln zur dreissigstelligen logarithmischen Rechnung," Leipzig. For *natural* logarithms and anti-logarithms to thirty places of decimals, the tables giving thirty-three places, independently calculated and verified. This is a most ingenious transformation of the positive numerical radix effected by subtracting the logarithmic series from its first term, so that instead of placing nat. log $1\cdot 0_m r$ against the number $1\cdot 0_m r$ in the radix, Professor Hoppe places $\cdot 0_m r -$ nat. log $1\cdot 0_m r$ against it. This transformed radix is calculated from $r=1$ to $r=9$, and $m=1$ to $m=15$ to thirty-three places. The calculation is consequently an alteration of Flower's reflected rule, adapted to natural logarithms, by which many figures are saved. It is probably, therefore, the shortest rule yet discovered. A reversed process gives the number. Table IV gives a multiplier of at most two digits, or a divisor of at most one digit, by which any number can be reduced to the form $\cdot 9\ldots$, which overcomes the principal difficulty in the use of Flower's reflected rule.

1877. *Namur*, A. "Tables de Logarithmes à 12 Décimales jusqu'à 434 milliards, avec preuves," Brussels. This is for tabular logarithms only, and depends upon the properties of logarithms nearly equal to the modulus, to which all others are reduced by appropriate factors. After this reduction the work is simple, no division being required, but I find the tables complicated, and very likely to produce error in consultation. The process is adapted only to tabular logarithms.

From this list it will be clear that the improved bimodular method of my former paper, and the potential radix which follows, have not been previously proposed.

By the *positive potential radix* of natural logarithms, I mean a table containing 10^r, 2^r, $(1{\cdot}1)^r$, $(1{\cdot}0_m1)^r$ and their natural logarithms generally from $r=1$ to $r=10$, but for 2^r it suffices to go to $r=3$, and for $(1{\cdot}1)^r$ to $r=8$, and from $m=1$ to $m=$ any required amount. By the *negative potential radix* of natural logarithms, I mean a table containing the numbers $(1-{\cdot}0_m1)^r$ within the same limits, and the negatives of their natural logarithms. If the improved bimodular method of my former paper be used, the number of places which can be determined from ${\cdot}0_m1$ as a quotient without correction is $3m+3$. By any other method we cannot secure more than $2m$ places. I was led to the construction of a potential radix by the bimodular method. In the case of the numerical radix, the ratio of any two consecutive numbers $1{\cdot}0_mr$ for a constant m and variable r, continually diminishes, but sufficient was gained for the action of the method, if the ratio remained constant, that is, if the consecutive numbers were the consecutive powers $(1{\cdot}0_m1)^r$, having the constant ratio $1{\cdot}0_m1$. Again, as such a power is very nearly equal to a number $1{\cdot}0_mr$ in the numerical radix, that is, as $1+r\times{\cdot}0_m1+\frac{1}{2}r\,(r-1)\times({\cdot}0_m1)^2+\ldots$ is very nearly $=1+r\times{\cdot}0_m1$, it became easy by the action of the method to obtain the numerical from the potential radix. The same is true for the negative radixes. Although from a potential radix the logarithm of a number could be obtained with the same accuracy as from a numerical radix, yet the process is much longer with the former, and hence it appears that the real use of the potential radix is to calculate the numerical radix. This is still more the case for the negative potential radix, which does not succeed in diminishing the work at all, and is here simply introduced for calculating the very useful negative numerical radix.

The calculation of—

$$\text{nat. log}\,(1+{\cdot}0_m1)={\cdot}0_m1-\tfrac{1}{2}\times{\cdot}0_{2m+1}1+\tfrac{1}{3}\times{\cdot}0_{3m+2}1-\ldots$$

$$-\text{nat. log}\,(1-{\cdot}0_m1)={\cdot}0_m1+\tfrac{1}{2}\times{\cdot}0_{2m+1}1+\tfrac{1}{3}\times{\cdot}0_{3m+2}1+\ldots$$

is very easy, even when $m=0$, that is for $1\pm{\cdot}1$, although in that case tedious, and is easier the larger m is. It is better to calculate these logarithms as checks for all values of m required, but it is actually not necessary to calculate more than that for the largest value of m to the requisite number of places. Thus, to fifty-two places (a subscript number denoting the number of times that the digit to which it is appended has to be repeated)—

$$\left\{\begin{array}{l} \text{nat. log}\,1{\cdot}0_41={\cdot}0_5\;9_5\;50_4\;3_5\;0\;83_3\;53_4\;16_4\;809\;52_2\;5_2\;9534\;920\;54 \\ -\text{nat. log}\,(1-{\cdot}0_41)={\cdot}0_4\;10_5\;50_4\;3_5\;583_8\;50_4\;1427\;583\;928\;682\;5407 \end{array}\right.$$

$$\left\{\begin{array}{l} \text{nat. log}\,1{\cdot}0_{14}1={\cdot}0_{15}\;9_{10}\;50_{14}\;3_7 \\ -\text{nat. log}\,(1-{\cdot}0_{14}1)={\cdot}0_{14}\;10_{15}\;50_{14}\;3_7. \end{array}\right.$$

The first pair would give a potential radix determining logarithms to at least twelve decimal places without correction. The second pair would give one determining logarithm with at least forty-two places correct, and generally many more.

Having found nat. log $(1\pm\cdot0_m1)$ for the extreme value of m, proceed thus:—Form $(1\pm0\cdot_m1)^r$ up to $r=10$, and the corresponding logarithms. Both operations are performed by simple addition or subtraction. Then find nat. log $(1\pm\cdot0_{m-1}1)$ either direct from the series or by the improved bimodular method from the next least $(1\pm\cdot0_m1)^9$ and next greater $(1\pm\cdot0_m1)^{10}$, of which the latter will give more places, or by both methods, to check the work. Then find the numerical radix for the stage $1\pm\cdot0_mr$ from the potential radix to this extent. Next proceed with the potential radix for the stage $(1\pm\cdot0_{m-1}1)^r$ whence derive the numerical radix for this stage, and so on till we obtain $(1\cdot1)^8$, which is just a little larger than 2, and from which nat. log of 2 and its powers may be found, which will include any number, however great.

In order to make this clear, I give a short positive and negative potential radix to twenty-one places of decimals up to $1\cdot0_21$, which with corrections (obtained from a table of cubes, like Barlow's, of numbers of four digits, from the formula $12c=x^3$ where x is the quotient to four significant places, and c is calculated also to four significant places) will give fourteen decimal places at least, and sometimes more. I then show the mode of calculating the numerical radix from it. It must be remembered that the nat. log. $1\cdot001$ is approximate, the digits after the eighteenth are 16681 in place of 167. Hence the tenth multiple will not have after the eighteenth place 670, but 668, and similarly in other cases. The three last places are given to avoid such errors and make eighteen places perfectly correct.

Here we may suppose that nat. logs of $1\cdot0_21$, $1\cdot01$, $1\cdot1$, and $1-\cdot0_21$, $1-\cdot01$, have been calculated directly from the formula. Then the powers of these numbers, and the multiples of their logarithms are obtained by simple addition and subtraction, and the potential radix is constituted except as regards 2 and 10. The calculation of nat. logs of 2, 3, 5, 7, 10 and $1\div$ nat. log 10 to upwards of 260 decimal places, by independent methods, by Professor J. C. Adams ("Proceedings," vol. 27, p. 92, for 7th February, 1878) obviates any necessity for the separate calculation of them by the present or any other method, but they could be calculated by this method, if it were necessary.

The next step is to calculate the numerical radix, or nat. log $(1\pm\cdot0_mr)$. Take for example $1\cdot004$, which is very slightly less than $(1\cdot001)^4$ of which the nat. log is known. Then my improved bimodular method, suppressing the details of the division, gives—

Positive Potential Radix.

No.	Powers of 1·001.	Their natural logarithms.
1	1·001	·000 999 500 333 083 533 167
2	1·002 001	·001 999 000 666 167 066 334
3	1·003 003 001	·002 998 500 999 250 599 501
4	1·004 006 004 001	·003 998 001 332 334 132 668
5	1·005 010 010 005 001	·004 997 501 665 417 665 835
6	1·006 015 020 015 006 001	·005 997 001 998 501 199 002
7	1·007 021 035 035 021 007 001	·006 996 502 331 584 732 169
8	1·008 028 056 070 056 028 008	·007 996 002 664 668 265 336
9	1·009 036 084 126 126 084 036	·008 995 502 997 751 798 503
10	1·010 045 120 210 252 210 120	·009 995 003 330 835 331 670

No.	Powers of 1·01.	Their natural logarithms.
1	1·01	·009 950 330 853 168 082 848
2	1·020 1	019 900 661 706 336 165 696
3	1·030 301	·029 850 992 559 504 248 544
4	1·040 604 01	·039 801 323 412 672 331 392
5	1·051 010 050 1	·049 751 654 265 840 414 240
6	1·061 520 150 601	·059 701 985 119 008 497 088
7	1·072 135 352 107 01	·069 652 315 972 176 579 936
8	1·082 856 705 628 080 1	·079 602 646 825 344 662 784
9	1·093 685 272 684 360 901	·089 552 977 678 512 745 632
10	1·104 622 125 401 204 510 01	·099 503 308 581 680 828 480

No.	Powers of 1·1.	Their natural logarithms.
1	1·1	·095 310 179 804 324 860 044
2	1·21	·190 620 359 608 649 720 088
3	1·331	·285 930 539 412 974 580 132
4	1·464 1	·381 240 719 217 299 440 176
5	1·610 51	·476 550 899 021 624 300 220
6	1·771 561	·571 861 078 825 949 160 264
7	1·948 717 1	·667 171 258 630 274 020 308
8	2·143 588 81	·762 481 438 434 598 880 352

Negative Potential Radix.

No.	Powers of 1 − ·001.	Negatives of their natural logarithms.
1	·999	·001 000 500 333 583 533 500
2	·998 001	·002 001 000 667 167 067 000
3	·997 002 999	·003 001 501 000 750 600 500
4	·996 005 996 001	·004 002 001 334 334 134 000
5	·995 009 990 004 999	·005 002 501 667 917 667 500
6	·994 014 980 014 994 001	·006 003 002 001 501 201 000
7	·993 020 965 034 979 006 999	·007 003 502 335 084 734 500
8	·992 027 944 069 944 027 992	·008 004 002 668 668 268 000
9	·991 035 916 125 874 083 964	·009 004 503 002 251 801 500
10	·990 044 880 209 748 209 880	·010 005 003 335 835 335 000

No.	Powers of 1 − ·01.	Negatives of their natural logarithms.
1	·99	·010 050 335 853 501 441 184
2	·980 1	·020 100 671 707 002 882 368
3	·970 299	·030 151 007 560 504 323 552
4	·960 596 01	·040 201 343 414 005 764 736
5	·950 990 049 9	·050 251 679 267 507 205 920
6	·941 480 149 401	·060 302 015 121 008 647 104
7	·932 065 347 906 99	·070 352 350 974 510 088 288
8	·922 744 694 427 920 1	·080 402 686 828 011 529 472
9	·913 517 247 483 640 899	·090 453 022 681 512 970 656
10	·904 382 075 008 804 490 01	·100 503 358 535 014 411 840

No.	Powers of 2.	Their natural logarithms.
1	2	·693 147 180 559 945 309 417
2	4	1·386 294 361 119 890 618 834
3	8	2·079 441 541 679 835 923 252

No.	Powers of 10.	Their natural logarithms.
1	10	2·302 585 092 994 045 684 018
2	100	4·605 170 185 988 091 368 036
3	1000	6·907 755 278 982 137 052 054
4	10000	9·210 340 371 975 182 736 072
&c.	&c.	&c.

1·004	a
1·004 006 004 001	$b=(1·001)^4$, next greater
2·008 006 004 001	$b+a$, divisor
12 008 002	$2(b-a)$, dividend
·000 005 980 062 796 661 85	$2(b-a)\div(b+a)$, quotient
$·0_{16}$ 17 82	correction$=\frac{1}{12}\times(·0_5 5980)^3$
·000 005 980 062 796 679 67	$\log b - \log a$
·003 998 001 332 334 132 67	$\log b$
·003 992 021 269 537 453 00	$\log a = \log b - (\log b - \log a)$

The result is correct to the last or twentieth place. If we had formed log a from the next less or $(1·001)^3$, the difference between the numbers would have been so large that the result would have been correct to thirteen places only, and we should have required higher stages in the radix to obtain twenty places. Hence the nearest number should always be selected.

To find —nat. log (1—·004)=—nat. log ·996, we should deduce it from—nat. log. $(1-·001)^4$, and as the difference in this case, which is always the approximate quotient, and hence logarithm, is less than $·0_5 6$, and $\frac{1}{12}\times(·0_5 6)^3=·0_{16}18$, we should obtain sixteen places without correction, and four with correction, or twenty places in all.

We thus proceed to form the whole of this stage of the numerical radix, but we cannot obtain twenty places in all cases. Thus for 1·009, the difference from $(1·001)^9$ is $·0_4 36084$, the quotient is $·0_4 3508$, and the correction$=\frac{1}{12}\times(·0_4 3508)^3=·0_{14}3594$, so that we should obtain only eighteen places, that is to say, although the potential radix is calculated to twenty-one places, it will not furnish a numerical radix of more than eighteen places when we begin with the stage $1·0_2 1$, and hence will not give logarithms of general numbers to more than sixteen places certain.

In the stage 1·01 the radix of that stage will not furnish so many places, and we have to reduce to the preceding stage, which is now supposed to be fully calculated for both the positive and negative numerical radixes. Thus for nat. log 1·04 as derived from 4 nat. log 1·01, the difference is $·0_3 6040$, giving correction $·0_{10}1836$, and hence fourteen places. But on dividing by 1·04 we obtain $1·0_3 58$ . . . and the difference from $1·0_3 6$, of which the natural logarithm in a preceding stage is known, is $·0_4 2$ . . ., the logarithm of which can be found to eighteen places. If more still were required we should divide by $1·0_3 5$, obtaining $1·0_4 8$ . . . of which we can find the logarithm through that of $1·0_4 8$, to twenty places at least. Hence if the potential radix has been commenced at a sufficiently high stage and to a sufficient number of decimal places, a numerical radix for natural logarithms can be calculated to any number of places, and from it the natural logarithm

of any number, such as the modulus of any other system of logarithms can be found, and its reciprocal, whence the radix for that system can be calculated by simple multiplication. This is sufficient to show the practicability of the present method, and generally the comparatively small trouble which it would occasion for the first construction of logarithmic tables.

VI. "On the Influence of Temperature on the Musical Pitch of Harmonium Reeds." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received January 17, 1881.

In my "Notes of Observations on Musical Beats," I stated ("Proc. Roy. Soc.," vol. 30, p. 532) that the influence of temperature on harmonium reeds was, so far as I was aware, unknown. Since then I have made some observations which at least approximately determine it, but there are so many sources of small errors (stated below) that still more uncertainty must attach to the results, than to the determination of the influence of temperature on the pitch of tuning-forks (*ibid.*, p. 523). Roughly we may say that the pitch of harmonium reeds is affected in the same direction as that of tuning-forks (heat flattening and cold sharpening), and very nearly to twice the amount, that is, by about 1 in 10,000 vibrations for each degree Fahrenheit. The following is the process pursued with the exact figures obtained:—

Towards the end of November, 1879, in the South Kensington Museum, with artificial temperatures (observed in each case) varying from 53° to 60° F. on different days, I determined the beats which all the reeds of Appunn's treble tonometer (*ibid.*, p. 527) made with Scheibler's forks (*ibid.*, p. 525). On 1st September, 1880, and again on 3rd September, 1880, at constant natural temperatures of 73° and 79° F. respectively, I took the beats of twelve of the reeds (the same on each occasion) with the same forks of Scheibler with which I had measured those reeds in November, 1879.

It is, of course, impossible to say whether either forks or reeds were precisely of the same temperature as the air. The reeds were inclosed in the wooden chest of the tonometer, which had been reposing in a glass wall-case in the same room during he night, and might not have fully acquired the general steady temperature of the room. The beats for each reed were counted 10 times each for 10 seconds, with each of two, and sometimes three forks, and the mean of each set of beats was employed. The known pitch of the forks at 59° F. (*ibid.*, p. 525) was then reduced to the temperature of the observation on the supposition that the number of vibrations altered by 1 in 20,000 for

1° F. (*ibid.*, p. 523). By adding or subtracting the mean of the observed beats from this calculated pitch of the fork, the pitches of the harmonium reeds at those temperatures were determined, and the mean of all the determinations for each reed was taken. In all cases I calculated to two places of decimals, but the second place cannot be depended on when counting the beats; and as the result is considerably affected by the second place, the process is not so satisfactory as could be wished, and must be regarded as only preliminary. The loss of pitch in proceeding from the lower to the higher temperatures thus determined was divided by the number of vibrations at the lower temperature, and also by the number of degrees F. of difference of temperature. The result or coefficient of temperature, being the alteration for 1 vibration and 1° F., would serve to reduce one pitch to the other, on the supposition, which cannot be more than approximately correct, that the alteration for temperature is uniform, and is the same for reeds of very different pitches and makes. Such a coefficient is, however, clearly better than none at all, and is especially useful in determining pitch by Appunn's instrument.

In the following table the number of the reed is that marked on Appunn's treble tonometer (*ibid.*, p. 527). The "pitch" means the number of double vibrations in a second made by the reed, on the three occasions of observation already mentioned, and as the temperature was variable during the first observations, made on different days, though constant on the same day, this is annexed in a separate column. As harmonium reeds are subject to rather sudden small alterations from causes not yet investigated, it is not possible to be perfectly sure that all the reeds would have shown precisely the same pitch at the same temperature for observations made at intervals of more than nine months. I am inclined to think that reeds 22 and 23 must have so altered. In other observations on reed 22, made 14th July, 1880, at 71°·5 F., I obtained practically the same results as here, differing from those of all the other reeds. The observations on that day were not sufficiently numerous nor exact to be here recorded, but they agree very well with those now given. The flattening of the reeds for each increase of temperature is quite unmistakable, even in the passage from pitch II to III, with a difference of only 6° F. The pitch III of reed 26 is certainly a bad observation, as the results of the determinations by the two forks differed much more than usual, and it should, therefore, be thrown out of consideration. Altogether for such a small difference as 6° F. the observations could not be made with sufficient accuracy to secure trustworthy results. Even for the greatest difference of temperature, 26° F., the difference of pitch never amounts to so much as ·9 vibration in a second for the reeds observed.

The three last columns give the coefficients of temperature arising from comparing these three pitches two and two, namely, I and II,

I and III, II and III. The results marked † should, I think, be rejected, because of the probable alteration in reeds 22 and 23, the badness of the observation on reed 26 pitch III, and the too great closeness of the temperatures for pitches II and III. Rejecting these, we obtain, as the mean of the results from I & II, and II & III respectively, the coefficients ·0000938 and ·0000930; so that either ·00009 or ·0001, that is, an alteration of 9 in 100,000 or 1 in 10,000 vibrations in a second for each change of 1° F. may be used with tolerable certainty, diminishing for heat and increasing for cold. Thus a tuning-fork like Scheibler's of 440 vibrations at 59° F. (his standard tuning A), an harmonium reed like Appunn's, and an open metallic flue-pipe of an organ, both in unison with the fork at the same temperature (the last having a coefficient of ·00104 acting in the opposite direction) would become 439·56, 439·12, and 449·15, at 79° F. respectively, so that it would be quite impossible to play the organ and harmonium together.

Table of the Pitch of Reeds at different Temperatures.

No. of Reed.	Pitch of the Reed.				Coefficient of Temperature.		
	I	at	II at 73°.	III at 79°.	From I and II.	From I and III.	From II and III.
0	254·04	55°·5	253·67	253·55	·000 0844	·000 0976	†·000 0723
3	266·11	,,	265·55	265·41	1192	1114	†816
9	289·96	55°	289·45	289·30	1159	948	†864
14	309 66	,,	309·18	309·06	861	807	†647
22	341·31	53°	340·91	340·76	†586	†620	†733
23	345·35	,,	344·88	344·65	†680	†780	†1112
25	358·50	,,	352·92	352·66	820	914	†1228
26	357·51	,,	356·90	†356·85	853	†1125	†0234
33	385·30	59°	384 77	384·64	945	960	†563
36	397·20	,,	396·75	396·55	809	818	†840
43	424·98	60°	424·42	424·23	1014	929	†746
46	436·71	,,	436·21	435·96	881	904	†955
Mean, rejecting results marked †,					·000 0938	·000 0930	

February 10, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Mountstuart Elphinstone Grant Duff was admitted into the Society.

The following Papers were read:—

I. "On the Influence of the Molecular Grouping in Organic Bodies on their Absorption in the Infra-red Region of the Spectrum." By Captain W. DE W. ABNEY, R.E., F.R.S., and Lieutenant-Colonel FESTING, R.E. Received February 5, 1881.

(Abstract.)

The authors describe the apparatus used by them in their research and their plan of mapping the absorption spectra, the results being given in wave-lengths. The source of light for obtaining a continuous spectrum was the incandescent positive pole of an electric light, the electricity being generated by an M. Gramme machine. The light was passed through tubes containing the fluid, and the absorption spectra photographed in the infra-red region.

The absorptions they met with they class as follows:—

1st. General absorption at the least refrangible end of the spectrum.

Lines .. { Fuzzy.
Sharp.

Bands .. { Both edges sharply defined.
One edge sharply defined.
Both edges less sharply defined.

The authors next discuss the causes of the different absorptions met with in various fluids. From experiment they show that a large number of lines which are formed in hydrocarbons containing no oxygen are common to substances containing hydrogen and no carbon, and that in carbon tetrachloride and carbon disulphide, no lines or bands are to be met with. By this eliminating process they deduce the fact that the presence of lines is due to the hydrogen in the bodies.

They show that the termination of the bands in liquids containing carbon, hydrogen, and oxygen corresponds with the position of these hydrogen lines. It therefore appears to them that the bands are in reality a blocking out of radiation between two hydrogen lines. By increasing the thickness of the fluid in front of the slit, the bands may be widened to another hydrogen line, each hydrogen line acting as a stepping-stone, or they may remain constant if both edges are defined, or they may be obliterated by general absorption. On the other hand, lines may be spread out to bands as the thickness of liquid is increased. When the thickness of the fluid is diminished the lines may disappear, and the bands become lines, or the bands may remain constant though fainter.

The authors then point out that each radical has its own definite absorption in the infra-red, and that such a radical can be detected in a more complex body. It also seems possible that the hydrogen which is replaced may be distinguished by a comparison with other spectra. They next point out coincidences between some of the lines obtained, the absorption spectra of the hydrocarbons, and the spectra of bodies containing no carbon with solar lines, from which they reason that at present it is not safe to infer that such lines in the solar spectrum are not necessarily due to water. Whether the lines mapped are due to hydrogen or not, it is perfectly evident that every organic body has a definite absorption spectrum which connects it with some series. The paper closes with an appendix giving tables of the bands and lines found in the following substances, of which also there are maps :—

Methyl iodide.
Ethyl iodide.
Propyl iodide.
Amyl iodide.
Phenyl iodide.
Ethyl bromide.
Amyl bromide.
Methyl alcohol.
Ethyl alcohol.
Propyl alcohol.
Isopropyl alcohol.
Isobutyl alcohol.
Pseudobutyl alcohol.
Amyl alcohol.
Diethyl ether.
Amyl ether.
Ethyl nitrate.
Ethyl oxalate.
Ethyl sulphide.
Aldehyde.
Paraldehyde.
Formic acid.
Acetic acid.
Propionic acid.
Isobutyric acid.
Valerianic acid.
Glycerine.
Benzene.
Phenyl bromide.
Benzyl chloride.
Nitrobenzole.
Aniline.
Dimethyl aniline.
Turpentine.
Olive oil.
Acetoacetic ether.
Diethyl acetoacetic ether.
Benzylethyl ether.
Methyl salicylate.
Cinnamic alcohol.
Phenylpropyl alcohol.
Dibenzyl acetic ether.
Allyl alcohol.
Allyl sulphide.
Anethol.
Citraconic anhydride.
Water.
Nitric acid.
Hydrochloric acid.
Sulphuric acid.
Ammonia.
Chloroform.

II. "Experiments undertaken during the Summer, 1880, at Yvoire (1,230 feet), Courmayeur (3,945 feet), and the 'Col de Géant' (11,030 feet), on the Influence of Altitude on Respiration." By WILLIAM MARCET, M.D., F.R.S. Received January 24, 1881.

Former communications on the influence of altitude on respiration have appeared in the "Proceedings of the Royal Society" for 1878 and 1879. I have continued the inquiry; and my present object is to give an account of my latest investigations on that subject.

I must beg leave to premise that a work of this kind is not free from difficulties; some of these are of a physiological nature, and refer to the task of taking into account the different circumstances bearing on the state of the body at the time of the experiment; respiration is, if I may so express it, so delicately balanced in its relations with the other functions, that any one of them becoming either quiescent or in a state of activity reacts immediately upon it.

Then the capacity of the bags for collecting the air expired gave some little trouble to determine accurately; and, moreover, the manipulations, which would require both care and practice to be carried on satisfactorily in a laboratory where all that can be wanted is at hand, were found much less easy to perform in the open air, at the summit of some high mountain, in a cold cutting wind, and where the omission of a trifling article, such as a cork or a piece of string, might render much labour useless. There is also a circumstance in a work of this kind having a tendency to detract from the value of its results, inasmuch as what is observed with one person may not apply to another. It is not easy to find many people ready to submit to a stay of several days in the Alps, at altitudes ranging up to 10,000 feet and above; and I know by experience that the increased length of time required to repeat the experiments on different persons may prove very inconvenient. This last summer, however, I was most fortunate in obtaining the company and help of a young gentleman from Geneva, M. Elie David, the assistant of the Professor of Natural Philosophy at the University of that town, Professor Wartmann. M. David submitted to experiment with great patience and much intelligence, and the results obtained on both of us may be accepted as perfectly trustworthy.

My method of investigation has been much improved since my last paper was communicated to the Royal Society, and the substitution of the vacuum process for the aspiration with water, in order to transfer air from the bag into the cylinder for analysis, has added not a little to the accuracy of the results. New and larger india-rubber bags than those used at first have been made, of improved material, and

their capacity has been determined with the greatest care by means of a gas-holder or bell-jar, having a scale of cubic feet divided into hundredths and tested as to correctness. I must beg to take this opportunity of returning my best thanks to Mr. Henry Sporne, Inspector of Gas Meters for the city of London, for his kindness in determining in my presence the capacity of the bag used in these experiments, by means of one of his beautifully graduated gas-holders, made for testing the delivery of gas meters. The capacity of others of my experimental bags was ascertained by the same means.

The bulk of air held by the bag employed in all the experiments referred to in the present communication was determined as follows:— The gas-holder having been filled with air, and the pressure of the instrument regulated to one inch of water, it was placed in communication with the bag laying flat and quite empty on a table. This bag was also connected through one of its necks and india-rubber tubing with another water-gauge. All these connexions being perfectly air-tight, the height of the air-holder was read off, and the air admitted into the bag from the holder. After some minutes the bag became fully distended, and the water began rising in the gauge connected directly with it. As soon as a pressure of one inch in the bag was attained, the delivery from the air-holder was read off; it had by that time all but done falling, moving downward very slowly, to the extent of a delivery of about one-hundredth of a cubic foot and then stopped; if the bag was then handled, one or two more hundredths of a cubic foot might be given out from the holder. One experiment gave a delivery of from 4 to 7·18 on the index = 3·18 cubic feet, read off as soon as the water in the gauge connected with the bag showed a pressure of one inch; on waiting two or three minutes, 3·19 cubic feet were read off; and, after handling the bag, the volume of air delivered into it rose to 3·22 cubic feet. In another experiment the reading on the holder was made to commence at 6, and when the bag was full it had fallen to 9·19, giving a bulk of 3·19 cubic feet of air delivered into the bag; after handling the bag, the air given out rose to 3·22 cubic feet. I have therefore taken 3·20 cubic feet, or 90·6 litres, as the correct estimation of the air contained in the bag under the pressure of one inch of water. In this last experiment the temperature of the air in the bag was ascertained to be the same as that of the room, and, moreover, the holder had been filled with air for some hours previously, so that it is very unlikely that its temperature should have been different from that of the external atmosphere; the temperature of the water in the gas-holder was a trifle below that of the room.

I proposed this last summer to make the Col du Géant (altitude 11,030 feet) my highest experimental station, as this spot was easy to reach with the necessary instruments, and a small wooden hut had been lately erected at the summit of the pass, where we could make

our residence during our stay at that place. The experiments were repeated at Yvoire near the Lake of Geneva (altitude 1,230 feet), and at Courmayeur (3,945 feet) at the foot of the Col du Géant on the Italian side. Hence, there were three series of data obtained in the course of about three months on two persons in good health, though very different in age, weight, and other respects. Two sets of experiments were made at Courmayeur, the first before ascending to the Col du Géant, and the second on our return; the object being to ascertain whether the influence of the sojourn at the higher station showed itself on our breathing for some time after we had left it. The experiments were invariably made in the sitting posture, and in the open air, away from every possible source of contamination with carbonic acid, and care was taken to remain perfectly quiet for a short time before collecting the air expired.

With the object of determining the amount of air expired within a given time, and the carbonic acid it contained, breathing was carried on through a face-piece connected with the india-rubber bag, and adapting itself perfectly over the mouth and nose; the face-piece was supplied with valves, so that fresh air was taken in at each inspiration, while the expired air was collected in the bag. A water gauge, communicating with the bag by india-rubber tubing, showed the pressure of the air it contained, and the bag was taken as full when the gauge exhibited a pressure of one inch. By means of a stop-watch registering minutes and seconds only, the time for filling the bag, and, therefore, for expiring 3·20 cubic feet or 90·6 litres of air, was accurately measured.

Immediately after the bag had been filled with air from the lungs, it was joined by india-rubber tubing with a cylinder made of very thick glass, and holding exactly two litres, which was placed in an upright position on a tripod stand. After exhausting the cylinder with an air pump, air was let into it from the bag, the operation being repeated, thereby ensuring that two litres of air had been withdrawn into the cylinder for analysis. Before closing the stop-cock and severing the connexion between the bag and cylinder a minute or two were allowed to elapse, and then the temperature of the air in the bag and in the cylinder was read off and noted. Care was taken to observe these temperatures while the bag and cylinder were communicating freely with each other, so as to allow of the equilibrium of pressure being established, should there be any difference between the temperature of the air in the bag and in the cylinder; sometimes the temperature was the same, but it frequently happened that the readings differed by 1° or 2°, the difference very seldom reaching 3° or 4°. A titered solution of baric hydrate was then introduced into the cylinder by a process described in a communication to the Chemical Society;*

* "Journal of the Chemical Society," July, 1880.

finally, the combination of the carbonic acid was completely effected by moving about the cylinder, so as to make the alkaline solution run up and down the inside of the glass for a period of ten minutes, this lapse of time having been ascertained experimentally to be amply sufficient. The milky fluid was withdrawn into a small bottle, carefully labelled, holding about 100 cub. centims., the bottle was well corked, and the cork secured with paraffin wax so as to prevent effectually the admission of air. I analysed the contents of the bottle on my return to Yvoire.

The experiments, numbering eighty, have been disposed in a tabular form; they are distributed as follows:—

Fourteen were made on myself at Yvoire, and ten on M. David; eight on myself at Courmayeur before ascending the Col du Géant, and eight on returning to Courmayeur, nine on M. David at Courmayeur before ascending the mountain, and seven on his return to Courmayeur. Twelve on myself at the summit of the Col du Géant, and also twelve on M. David at the same station.

I shall beg to commence by inquiring into the influence of food and temperature upon our breathing.

Influence of Food.—In my former investigations, the direct influence of meals as increasing the amount of carbonic acid expired was, as a rule, well marked. The limited range of time that elapsed after a meal previous to an experiment, and the small number of experiments at each station in the present inquiry, preclude any but comparatively wide results being arrived at as to the direct influence food exerted on the formation of carbonic acid. Dr. Edward Smith* concludes that the greatest quantity of carbonic acid is expired from one and a half to two and a half hours after a meal; in the present experiments, the ingestion of food showed a direct influence in my case towards increasing the amount of carbonic acid expired, although we cannot find any definite tendency of the kind with M. David. At Yvoire, I expired the smallest quantity of carbonic acid between breakfast and luncheon, when the mean of four experiments undertaken during that period yielded 0·479 grm. The carbonic acid rose under the influence of a full luncheon, and attained a maximum of 0·572 grm. 1 h. 45 m. after that meal in the first series of experiments, and of 0·638 grm. 3 h. 2 m. in the second series. At Courmayeur, before ascending the Col, my minimum carbonic acid (0·449) was expired 4 h. 35 m. after breakfast, and the maximum (0·554) 2 h. 40 m. after luncheon. On our return to that station, my minimum (0·434 grm.) is expired at the latest experiment after breakfast, and my maximum (0·561 grm.) 3 h. 35 m. after luncheon. So far the results agree tolerably, for such a small number of experiments, with the influence food is usually found to exert on the formation and expiration of carbonic acid.

* "Phil. Trans.," 1859.

In M. David's case so much irregularity is observed as to the amount of carbonic acid expired in connexion with the food taken, that it is not possible to draw any inference as to the direct influence food exerted in this respect. He informs me he considers his digestion so slow that he does not bathe (in the Lake of Geneva) till three and a half or four hours have elapsed after a meal.

Influence of the Temperature of the Atmosphere on the Amount of Carbonic Acid Expired.—This influence was sharply defined, even for small differences in the temperature of the air, and it was found that at the same station as the atmosphere became warmer, the amount of carbonic acid expired fell precisely in an equal proportion. The first series of experiments on myself at Yvoire were made on the 27th and 28th of August; the temperature of the air was still high, and the mean reading of the thermometer in the shade during the experiment, was 69°·9 F. On that occasion, a mean of 504 mgrms. carbonic acid was found to be expired by myself per minute, at a mean period of 2 h. 46 m. after a meal. A second series of experiments upon myself at that same station was undertaken on the 20th, 21st, and 22nd October, yielding a mean of 572 mgrms. carbonic acid expired per minute; autumn was setting in, and the atmospheric temperature had fallen to a mean of 60°·3 at the time of the experiments; these were made at a mean period of 2 h. 35 m. after a meal, or nearly the same as in the first series. The circumstances attending the two series of experiments were therefore similar, with the exception of the atmospheric temperature, which was lower by 9°·6 on the second than on the first occasion. It is very interesting to observe, that the carbonic acid in the second series is just what might have been expected, if in an inverse proportion to the temperatures, these proportions being calculated would yield 0·493 grm., which is very nearly 0·504, or 0·584 mgrm., which is very nearly 0·573. If we now consider the experiments made on myself at Courmayeur, before ascending to the Col, they will be found to yield 494 mgrms. of carbonic acid with an atmospheric temperature of 76°·1, while the second series, undertaken on returning to Courmayeur, after our stay on the Col du Géant, gave 492 mgrms. at a mean temperature of 77°, the mean time the experiments were made after food being 2 h. 43 m. in one case, and 2 h. 29 m. in the other. In this instance, the mean temperature varies by less than 1°, and the carbonic acid expired is also as near as possible the same.

M. David's two series of experiments at Courmayeur, at a mean time of 2 h. 58 m. and 3 h. 34 m. respectively after a meal, gave for the first 727 mgrms. carbonic acid, with a mean atmospheric temperature of 71°·2, and for the second, after our return to Courmayeur again, 727 mgrms. carbonic acid, with a mean temperature of 72°·2, these figures harmonising in a remarkable degree.

If altitude, or a fall of atmospheric pressure, with which I couple increased dryness of the atmosphere, exerted no influence on the amount of carbonic acid expired, independently of temperature, the combustion within the body would be regulated by the temperature of the air only, all other circumstances supposed to be similar;* in that case the weight of carbonic acid at Yvoire and at Courmayeur, reduced to equal temperatures, would have been the same. We find, however, that such is not the case, and that, after reducing the carbonic acid for Yvoire to the temperature corresponding to that of Courmayeur, a marked excess of carbonic acid is expired at Courmayeur, and that this applies to both of us, as is clearly seen in the following table:—

Table showing the theoretical excess of CO_2 expired at Courmayeur over that expired at Yvoire (difference of altitude, 2,715 feet) for equal temperatures.

Myself (Courmayeur, 1st Series).

CO_2 at Courmayeur (1st Series)	0·494	t. air	76°·1
CO_2 at Yvoire (2nd Series)	0·572	t. air	60°·3
CO_2 at Yvoire (2nd Series) reduced for temperature	0·453	at	76°·1

Increase of CO_2 expired at Courmayeur 8·3 per cent.

Myself (Courmayeur, 2nd Series).

CO_2 at Courmayeur (2nd Series)	0·492	t. air	77°·0
CO_2 at Yvoire (2nd Series)	0·572	t. air	60°·3
CO_2 at Yvoire (2nd Series) reduced for temperature	0·448	at	77°·0

Increase of CO_2 expired at Courmayeur 8·9 per cent.

M. David (Courmayeur, 1st Series).

CO_2 at Courmayeur (1st Series)	0·727	t. air	71°·2
CO_2 at Yvoire	0·776	t. air	61°·6
CO_2 at Yvoire reduced for temperature	0·671	at	71°·2

Increase of CO_2 expired at Courmayeur 7·7 per cent.

M. David (Courmayeur, 2nd Series).

CO_2 at Courmayeur (2nd Series)	0·727	t. air	72°·2
CO_2 at Yvoire	0·776	t. air	61°·6
CO_2 at Yvoire reduced for temperature	0·662	at	72°·2

Increase of CO_2 expired at Courmayeur 8·9 per cent.

* There exist apparently other circumstances besides food and temperature of the air exerting an influence on the amount of carbonic acid expired, but neutralising each other in a series of experiments.

This table shows, in my own case, a theoretical excess of 8·3 per cent. and 8·9 per cent. of carbonic acid expired for the first and second series at Courmayeur respectively over my second series at Yvoire, assuming the temperatures to be the same at both stations. In M. David's case, the theoretical excess of carbonic acid expired at Courmayeur over that expired at Yvoire, for similar temperatures, was found to be 7·7 per cent. and 8·9 per cent. for the two Courmayeur series respectively. It follows that, for equal mean temperatures, even such a small difference of altitude as 2,715 feet would have given in these experiments a mean increase of carbonic acid of 8·6 per cent. in my case and 8·3 per cent. with M. David. M. Mermod,* who experimented with equal mean temperatures at a difference of altitude of 3,143 feet (from 142 metres to 1,100 metres) obtained an increase of carbonic acid expired by 6·7 per cent. at his highest station, which is very near to my result although slightly lower. His experiments were all made fasting, between 7 and 8 o'clock in the morning, and it is not unlikely that his comparatively low increase of carbonic acid at the highest station was due in some measure to his being in want of food at the time, and unable to generate exactly the increased amount of carbonic acid required to make up for the altitude.

It may be asked, why should the body make more carbonic acid at increasing altitudes, and under similar atmospheric temperatures? The reply is, that the evaporation from the skin and lungs must be greater under a lower pressure and drier atmosphere, giving rise to a production of cold. The air was exceedingly dry while we were at Courmayeur; on July 21st, at 12.30 p.m., the readings of dry and wet bulb thermometers were 78° and 62° respectively, giving a relative atmospheric humidity of 39 per cent.; the same day, at 2.15 p.m., the readings were 78° and 59°, corresponding to 32 per cent. of relative atmospheric humidity. Under such a dry air there must have been a considerable evaporation from the skin and lungs, cooling the body and thereby more than making up apparently for the absorption of heat, the result being an excess of carbonic acid expired.

The twelve experiments for each of us on the summit of the Col cannot be viewed in the same light as the others, because although the cold we were subjected to under such a low atmospheric pressure was comparatively great, and, indeed, keenly felt, still we both gave out less carbonic acid than at Courmayeur instead of more. This circumstance will find presently a ready explanation; in the meantime I shall propose to divide the twelve experiments on the Col into two groups of six for each of us respectively, one group corresponding to the lowest and the other to the highest atmospheric temperatures. The

* "Bulletin de la Soc. Vaudoise des Sc. Nat.," vol. xv, 1877.

mean temperature and corresponding mean weight of carbonic acid expired will be as follows:—

		grm.		°
For myself....	CO_2 expired	0·411	.. mean temperature	39·8
	CO_2 expired	0·454	.. mean temperature	43·5
For M. David..	CO_2 expired	0·605	.. mean temperature	40·9
	CO_2 expired	0·614	.. mean temperature	45·8

so that rather less carbonic acid was expired at the lower temperatures. Consequently, not only the total mean carbonic acid expired at the Col was found to be lower than the total mean either at Courmayeur or at Yvoire, but moreover during our stay on the Col the coldest weather was attended with the least formation of carbonic acid for both of us, which is quite in opposition to what might have been expected. This circumstance, however, is easily accounted for. After spending two or three days at Courmayeur were the sun was very hot, the thermometer (sheltered from the sun under or inside a bush) rising to 80°, we started for the summit of the Col without having in any way prepared our functions to the sudden change of climate to which they were going to be subjected. We were indeed fresh from the plains in the hottest time of the year, and it was as taking a leap from the middle of summer into the midst of winter. We left Courmayeur one afternoon, and slept at the inn about 3,000 feet higher; next morning an early start brought us in good time to the hut at the summit of the Col, in the midst of snow slopes and glaciers. The floor of the hut was found covered in a great measure with ice and snow, which had to be removed with axes, while the small stove in one corner was nearly buried in snow, and so imbedded in ice that it took us some time before we could get it free. People who cross this pass, one of the most beautiful in the Alps, spend an hour at the hut and often less; few travellers sleep in it, but this was to be our home for at least three days. The wind was cold and cutting, and a thick greatcoat proved quite insufficient. M. David had taken with him a coat lined with fur, but even that was not equal to keeping up a comfortable warmth. The air was very dry; on the 19th July at 10 a.m. the readings of the dry and wet bulb thermometers were 42° and 33°, giving a relative humidity of 44 per cent. (Glaisher's tables), so that the atmosphere possessed a great capacity for moisture, and therefore an increased power of cooling down the body. The experiments were to be made out of the welcome sun's rays, and, with this object in view, I had brought up a wooden shed which had already done useful work on an expedition to the Peak of Teneriffe; this shed or roof was erected on four poles, kept in their places by tent ropes and pegs. It may be readily understood that sitting under the shed with the back resting against a rock and facing the beautiful glacier which descends directly from the Col, was

not conducive to warmth. Food, good, abundant and substantial, would have offered the best means to counteract the cold, but our supply was scanty, as we had been disappointed in the expectation of obtaining provisions at the half way house, the Pavillon du Mont Frety, and even had food been abundant, want of appetite and inability to digest with both of us would have prevented our taking advantage of it. As it was, we lived mostly on soup made with extract of beef and preserved julienne, bread and cheese, coffee and tea with condensed milk, some chocolate, and a small quantity of meat from tins. The nights we felt bitterly cold and uncomfortable, and we could get but very little sleep. No wonder that under such depressing circumstances we were unable to make in our bodies the amount of heat required to meet the occasion; the result, in fact, was, as I have already stated, a diminished combustion in both of us.*

I now wish to offer a few remarks on the difference in the weight of carbonic acid expired by myself and M. David respectively at the several stations. M. David is 25 years of age, weighs 11 stone 7 pounds (73 kilos.), and measures round the chest 37·6 inches, while I am 52 years of age, weigh nearly 10 stone 8 pounds, and measure 34·5 inches round the chest. M. David expired more carbonic acid than I did, though not in proportion with the excess of weight of his body. If the figures showing the carbonic acid expired by each of us be reduced to the same temperature for each station, the fixity of the excess of carbonic acid expired by M. David, not only at the different stations respectively, but also throughout the whole of the present inquiry, becomes quite obvious.

At Yvoire my first series of experiments yield 0·504 CO_2 at 69°·9; and M. David's only series, 0·776 CO_2 at 61°·6, the reduction to the same temperature giving an excess of 26·3 per cent. for M. David. My second series of experiments at Yvoire, compared with M. David's single set of experiments at that station, shows a difference of atmospheric temperature from 60°·3 to 61°·6; after applying the reduction, the excess of carbonic acid expired by this gentleman will be found to amount to 27·9 per cent., or within narrow limits of the percentage first obtained.

At Courmayeur, for the first series of experiments, after applying the correction for temperature, the excess of carbonic acid expired by M. David was found to be 27·4 per cent. The second series at Courmayeur showed an excess of carbonic acid expired (duly corrected) by M. David amounting to 27·9 per cent., or very nearly the same as in

* My experimental baggage consisted of a trunk weighing 40 kilos. (88 lbs.), a basket weighing 35 kilos. (77 lbs.), and the wooden shed; three porters from Courmayeur carried up the whole of this baggage from the Pavillon du Mont Frety; I engaged a fourth for taking the baggage down. The work of these porters was a remarkable feat of bodily strength and mountaineering.

the corresponding series. It is impossible to apply any correction for temperature in the experiments made on the Col du Géant. The mean temperature for each of us is not very dissimilar, being 41°·7 in one case and 43°·4 in the other. The excess of carbonic acid expired at that station by M. David amounted to 28·6 per cent.

These various percentages were alike in a remarkable degree, as will be seen by placing them in a tabular form:—

Excess of Carbonic Acid expired by M. David (after reduction for Temperature.)

At Yvoire.

M. David's single series	26·3 per cent. excess over my 1st series.	
	27·9 „ „ 2nd „	

At Courmayeur.

1st series........	27·4 per cent. before ascending.
2nd series.......	27·9 „ after returning.

On the Summit of the Col.

28·6 per cent.

Total means .... 27·6 per cent.

It may, therefore, be concluded that the mean excess of carbonic acid expired at each station by M. David over that I gave out was very nearly the same, and amounted to a mean of 27·6 per cent.

One of the most interesting results of the present inquiry relates to the volume of air expired per minute, and its relation to the carbonic acid it was found to contain. This is shown in the following table:—

Self. Volume air (reduced) expired per minute.	*Self.* Volume air (reduced) expired for 1 grm. CO_2.	*M. David.* Volume air (reduced) expired per minute.	*M. David.* Volume air (reduced) expired for 1 grm. CO_2.
	Litres.		Litres.
Yvoire (1,230 *feet*).		*Yvoire.*	
1st Series 8·04........	15·5	1st Series 10·65	13·7
2nd Series 8·39........	15·4		
Courmayeur (3,945 *feet*).		*Courmayeur.*	
Before ascending . 7·05..	14·3	Before ascending 10·29 .	14·2
After returning .. 7·07..	14·4	After returning 11·28 .	15·5
Summit of the Col (11,030 *feet*).		*Summit of the Col.*	
5·82..	13·5	7·67..	12·6

It will be observed, first of all, on considering these figures, that both in my case and M. David's much less air, reduced, or a smaller weight of air, was expired per minute, and consequently breathed at 11,030 feet above the sea than at 1,230 feet; this difference was in my case by 29·2 for 100 expired at 1,230 feet, and with M. David's by 28·0 per cent.; and it is not a little remarkable that the difference per cent. in both cases should be all but the same. There is also a falling off of the volume of air (reduced) I exhaled at Courmayeur, but M. David expired much the same amount at both stations. In my former experiments at Teneriffe and in the Alps I also observed a reduction in the volume of air expired (reduced) at high altitudes, so that there can be no doubt that, although the actual *volume* of air breathed is greater because of its being rarefied, still a smaller *weight* of air is breathed within a given time in the mountains than in the plains.

I shall now beg to consider the volume of air expired for 1 grm. of carbonic acid at our various stations—a subject interesting both in a physiological and medical point of view. In my case, the proportion of air breathed for 1 grm. CO_2 expired falls steadily from our lower to our higher station; or, in other words, at Courmayeur I required a weight of 8·6 per cent. less air to make 1 grm. of carbonic acid than at Yvoire; and at the summit of the pass I inhaled a weight of air as much as 14·0 per cent. smaller to produce 1 grm. of carbonic acid than at the lowest station. In M. David's case there is a decrease of 8 per cent. in the weight of air expired at the summit of the Col for 1 grm. CO_2 compared with that expired near the Lake of Geneva. At the intermediate station (3,945 feet), however, there is a slight increase; the rule remains, nevertheless, I consider, as well proved, that at certain altitudes, which may vary with different persons, a smaller weight of air is required to yield the necessary oxygen for producing in the body the same weight of carbonic acid; this law also held good on my former experimental stations. It accounts, in my mind, for the immunity against consumption met with amongst the inhabitants of towns and villages at elevations beyond 4,000 or 5,000 feet above the sea, and for the beneficial influence exercised in some cases on the progress of phthisis by such high places as Davos in Switzerland. At these stations a smaller weight of air is required for the production of carbonic acid than in the plains, which means that the air breathed passes through the substance of the lungs into the blood more readily at certain altitudes than it does nearer the sea-level; and, consequently, where the pulmonary organ is unequal to a free diffusion of air through its tissue, as is the case in consumption, and probably in old-standing bronchitis, such an influence of altitude on respiration is likely to produce a favourable change. M. Mermod, whose interesting inquiries on the effects of altitude upon respiration I

have previously quoted, found that a smaller weight of air and more carbonic acid were expired at his higher than his lower station, showing, as I have done from my experiments, that at increasing altitudes carbonic acid requires the inspiration of a smaller weight of air to be formed in the body than nearer to the sea level.

A few remarks only are left to be made on the rate of breathing and volume of air taken into the lungs per respiration at the various stations. The expirations were counted during three minutes, as each experiment was being commenced: the person submitting to experiment breathing freely in the open air and in the sitting posture. The mean rate of breathing was much the same for M. David and myself at Yvoire and at Courmayeur, before ascending to the Col, there was consequently no appreciable difference for a rise in altitude of 2,265 feet. The influence of our stay on the Col, where M. David's respiration increased in frequency, apparently showed itself on this gentleman on our return to Courmayeur, as a reaction appeared to set in at the time, and the number of expirations fell from 20·6 per minute on the Col to 8·5, 7·5, 9·5, and 9·7 per minute, rising afterwards to 10, 12·7, and 15·3. The cause of this change was apparently the fatigue of the increased rate of breathing during the three days we spent on the pass, requiring rest of the muscles of respiration. In my case there was also an increase, though not so great as in the former (11·6 to 15·6 per minute), in the frequency of breathing on the summit, which appeared to give rise to no reaction beyond a slight irregularity in the respiration on our return to Courmayeur. We arrived at that place from the Col on the 19th July, in the evening; M. David's experiments were begun on the 20th July, at noon, and continued that day, one only being made on the 21st. Two of my experiments were undertaken on the 20th and the remaining on the 21st, so that a longer time had elapsed in my cases since we had returned from the Col: this circumstance may explain the apparent absence of reaction in my own breathing. I do not ascribe, however, much physiological importance to these figures, but we were much struck with M. David's slow respiration at the time.

The mean volume of air per respiration (not reduced) was in my case as near as possible the same at the intermediate as at the lower station (1 litre Yvoire and 0·98 Courmayeur), but it became much smaller at the summit of the pass, where it fell to 0·64 litre. M. David expired a mean of 0·71 litre of air per expiration at the lower station, of 1·0 litre at the intermediate one, and 0·48 litre on the summit of the pass.

Conclusions.

1. The direct influence of food as increasing the amount of carbonic acid expired was obvious in my case in a general way, although

not in that of M. David. This result in M. David's case in no way invalidates the well-known influence of the ingestion of food as increasing the amount of carbonic acid expired, but is apparently due in a great measure to the comparatively small number of experiments, and to circumstances connected with a slow rate of digestion.

2. The influence of the temperature of the atmosphere on the amount of carbonic acid expired was well marked in both cases; the mean weights of carbonic acid expired at Courmayeur (3,945 feet) by both of us being the same respectively at the mean temperatures observed, which were equal in both series; but it was found that the amount of carbonic acid expired at the same station varied inversely with the change of temperature; the readings observed ranged from 60°·3 to 77° (Col du Géant not included).

3. For a difference of altitude of 2,715 feet between 1,230 feet and 3,945 feet, and a relative atmospheric humidity lower by about 31 per cent. for the higher station (70 per cent. at Yvoire), the mean weight of carbonic acid expired by each of us, reduced to the same temperature was found to be in excess at the higher over the lower station by 8·6 per cent. in my case and 8·3 per cent. in M. David's. This shows clearly the influence of altitude coupled with increased atmospheric dryness towards promoting combustion in the body.

4. The sudden change from a lower and very warm place to a very high and cold station, with a barely adequate diet and insufficient training, was productive on the present occasion of a decreased formation of carbonic acid in both of us, and a feeling of want of power to react against the external cold.

5. The amount of carbonic acid expired by each of us was not the same. M. David, a younger man than I am, gave out a total mean of 27·6 per cent. more carbonic acid than I did, calculating this excess from the carbonic acid we expired per minute reduced to equal temperatures; and moreover the excess exhibited much regularity at each station respectively. The mean surplus, without reduction for temperature, would have amounted to 30·9 per cent.

6. A smaller weight (volume reduced) of air is breathed within a given time in the mountains, at altitudes which may vary with different persons, than in the plains or low valleys, although the actual volume of air expired be larger under a lower barometrical pressure; and, moreover, a smaller weight of air is required by the body to produce within it a given weight of carbonic acid at certain elevations than near the sea level; this confirming my results obtained on former occasions.

7. At an elevation of 11,030 feet above the sea, and a mean temperature of 41°·7 and 43°·4 for each of us respectively, the rate of breathing was accelerated in both our cases, though rather less in mine

than in M. David's. His increase was by 39·5 per cent., and mine by 25·6 per cent. I had occasion to remark, in a paper published last year,* that my observations showed no increase in the frequency of respiration in the Alps, while in the sitting posture, between the altitudes of 1,230 feet (Lake of Geneva) and 8,115 (St. Bernard); but between 8,115 feet and 13,685 feet (summit of the Breithorn) the increase was in the following ratio:—To 8,428 feet, 20·4 per cent.; from thence to 10,899 feet, 12·7 per cent. beyond the latter; from that elevation to 13,685 feet, 6·3 per cent. more; altogether 39 per cent. The rate of breathing was not observed at that time quite in the same way as on the present occasion, as it was then reckoned while expiring into the bag, and not into the open air. I cannot anticipate, however, any actual difference in the results, as there is no reason why the rate of expiration into the bag should not be proportional at various altitudes to the rate of expiration into the open air at these same altitudes respectively.

I feel called upon to remark that the figures for the carbonic acid I expired in the experiments which form the subject of the present paper are higher than those reported in my earlier experiments, while the volume of air expired is rather largely increased in this last inquiry, which I am hardly prepared at present to account for. A series of experiments made on myself at Yvoire in autumn, 1879, yielded weights of carbonic acid much nearer to those last obtained. The weight of air expired for one gramme of carbonic acid, which had been somewhat smaller in the earlier experiments, rose in those of 1879 at Yvoire to a figure but slightly inferior to that obtained in the experiments of 1880.

* "On the Riviera, Madeira, the Canary Islands, and Davos, with reference to their Climate for Consumptive Invalids." "Edin. Med. Journ.," 1880.

Experiments at Yvoire on myself, 27th and 28th August, 1880. 1st Series.

Barometer taken at 28·662 inches=728 millims. Altitude, 1,230 feet=373 metres.

	Grm. CO_2 expired air per minute.	Air expired per minute (not reduced).	Air expired per min. reduced to freezing point and seaside pressure.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute (without face-piece).	Number of expirations per min. into bag, with face-piece.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
	Grm.	Litres.	Litres.	Litres.				Fah.	h. m.
1	0 448	7·71	6·80	15·2	11·3	9·0	0·86	68°·5	2 50 after breakfast, rather hungry.
2	0·572	9·81	8·66	15·1	13·0	10·4	0·94	71	1 45 after luncheon.
3	0·487	9·45	8·34	17·1	15·3	11·6	0·82	71	3 10 "
4	0·525	9·66	8·53	16·3	10·7	10·1	0·96	71	3 35 "
5	0·533	9 94	8·78	16·5	10·0	8·9	1·06	69	4 30 "
6	0·504	8·81	7·78	15·4	11·7	9·4	0·94	69	1 20 after breakfast.
7	0·458	8·36	7·38	16·1	11·7	9·3	0·90	70	2 10 "
Means	0·504	9·11	*8·04	15·9	12·0	9·8	0·93	69·9	2 46 after a meal.

Experiments at Yvoire, 20th, 21st, and 22nd October, 1880, on myself. 2nd Series.

	Grm. CO_2 expired air per minute.	Air expired per minute (not reduced).	Air expired per min. reduced to freezing point and seaside pressure.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute (without face-piece).	Number of expirations per min. into bag, with face-piece.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
1	0·522	9·14	8·27	15·8	10·3	9·1	1·00	58	1 43 after luncheon.
2	0·617	9·57	8·66	14·0	11·7	8·8	1·08	58	
3	0·505	8·94	8·09	16·2	10·3	8·4	1·06	62	3 0 after breakfast.
4	0·585	10·53	9·53	16·3	13·0	9·9	1·06	62	2 0 after luncheon.
5	0·638	10·12	9·16	14·4	10·7	..	..	61	3 2 "
6	0·549	10·53	9·53	17·4	10·0	8·5	1·24	59	4 25 "
7	0·591	8·83	8·00	13·5	10·3	8·2	1·08	62	1 5 after dinner.
Means	0·572	9·66	8·75	15·4	10·9	8·8	1·09	60·3	2 33 after a meal.
			Mean	of the two	series of	experiments.			
	0·538	9·38	8·39	15·5	11·4	9·3	1·00	65·1	2·39 after a meal.

* The temperature for reduction was the mean temperature of the air expired into the bag, when full, for each series respectively.

Experiments at Yvoire, 18th and 19th September, 1880, on M. David.

18th September, a fine day, cool air, wind S.W. moderate. 19th September, chilly, sunless morning, wind S.W. light.

	Grm. CO_2 expired per minute.	Air expired per minute (not reduced).	Air expired per minute, reduced to freezing point and seaside pressure.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute (without face-piece).	Number of expirations per minute into bag with face-piece.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
Sept. 18.								°	h. m.
1	0·664	10·30	9·26	13·9	13·3	15·6	0·66	65	1 2 after luncheon.
2	0·750	11·64	10·46	18·9	13·2	17·1	0·68	65	1 58 "
3	0·664	10·51	9·44	14·2	12·3	16·7	0·63	63	4 30 "
4	0·898	13·10	11·77	13·1	12·3	18·8	0·70	58	5 20 "
Sept. 19.									
5	0·770	11·62	10·44	13·6	12·7	18·1	0·64	57	0 35 after breakfast.
6	0·713	11·05	9·93	13·9	12·0	15·5	0·71	61	2 35 "
7	0·816	13·55	12·18	14·9	12·7	17·6	0·77	63	0 35 after luncheon.
8	0·825	11·59	10·41	12·6	12·3	16·1	0·72	63	2 5 "
9	0·829	12·47	11·20	13·5	13·5	17·3	0·72	62	3 1 "
10	0·832	12·64	11·36	13·7	13·0	15·0	0·84	58	3 55 "
Means	0·776	11·85	10·65	13·7	12·7	16·8	0·71	61·6	2 56 after a meal.

Experiments at Courmayeur, on myself, before ascending to the Col du Géant, July, 1880.

Barometer 25·948 inches=665·9 millims. Altitude 3,945 feet=1,202 metres. A beautiful day, light breeze, very hot afternoon.

	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced to freezing point and seaside pressure.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute into the open air without face-piece.	Number of expirations per minute into bag with face-piece.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
July 16.								Fah.	h. m.
1	0·496	8·61	6·88	13·9	14·0	8·4	1·03	69°·5	1 20 after breakfast.
2	0·493	8·64	6·91	14·0	13·0	8·0	1·08	73	2 5 „
3	0·465	8·78	7·02	15·1	13·7	8·5	1·03	76	3 40 „
4	0·473	9·10	7·27	15·4	10·7	8·3	1·10	76	4 10 „
5	0·449	8·49	6·78	15·1	10·3	7·7	1·10	75	4 35 „
6	0·491	8·43	6·74	13·7	..	7·3	1·15	79·5	1 20 after luncheon.
7	0·534	9·00	7·19	13·5	9·0	8·0	1·12	80	1 55 „
8	0·554	9·52	7·61	13·7	10·3	10·2	0·93	80	2 40 „
Means	0·494	8·82	7·05	14·3	11·6	8·3	1·07	76·1	2 43 after a meal.

Experiments at Courmayeur, on M. David, before ascending the Col du Géant.

Weather very fine, a few light clouds.

	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced to freezing point and seaside pressure.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute into the open air without face-piece.	Number of expirations per minute into bag with face-piece.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
July 15.								Fah.	h. m.
1	0·869	14·4	11·66	13·4	13·7	19·4	0·74	77°	2 45 after breakfast.
2	0·689	11·7	9·48	13·8	..	13·7	0·85	77	3 50 „
3	0·710	12·2	9·88	13·9	10·3	16·8	0·73	77	0 55 after luncheon.
4	0·675	12·3	9·97	14·8	..	16·9	0·73	76	1 27 „
5	0·841	13·6	11·02	13·1	12·0	14·5	0·94	70	2 17 „
6	0·664	12·1	9·81	14·8	12·3	16·9	0·72	68·5	2 55 „
7	0·680	12·1	9·81	14·4	11·0	16·9	0·72	66·5	3 23 „
8	0·673	12·1	9·81	14·6	12·8	17·3	0·70	66	4 22 „
9	0·744	13·8	11·18	15·0	12·3	16·3	0·85	63	4 50 „
Means	0·727	12·7	10·29	14·2	12·1	16·5	0·78	71·2	2 58 after a meal.

Experiments at Courmayeur on myself, returning from Col du Géant.

Barometer 25·948 inches=665·9 milims. Altitude 3,945 feet=1,202 metres. 21st July, fine day with light breeze, clouds moving from the west, a thunderstorm in afternoon.

	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute into open air (no face-piece).	Number of expirations per minute into bag.	Volume of air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
July 20. 1	0·487	9·32	7·46	15·3	13·7	10·7	0·87	Fah. 77°	h. m. 1 15 after luncheon.
2	0·546	9·04	7·29	13·4	16·7	10·0	0·90	78	2 15 „
July 21. 3	0·455	8·45	6·77	14·9	10·3	9·7	0·87	74	1 45 after breakfast.
4	0·444	7·89	6·27	14·1	10·3	8·9	0·89	79	3 0 „
5	0·434	8·02	6·37	14·7	10·3	8·8	0·91	80	3 40 „
6	0·536	9·40	7·50	14·0	15·3	10·2	0·92	78	1 55 after luncheon.
7	0·472	8·35	6·65	14·1	13·3	9·9	0·84	77	2 30 „
8	0·561	10·05	8·22	14·7	13·3	11·0	0·91	71	3 35 „
Means	0·492	8·82	7·07	14·4	12·9	9·9	0·89	77	2 29 after a meal.

Experiments at Courmayeur, returning from the Col du Géant, upon M. David.

20th July, hot and sultry.

	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced.	Air expired (reduced) for 1 grm. CO_2.	Number of expirations per minute into open air (no face-piece).	Number of expirations per minute into bag.	Volume of air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
July 20.								Fah.	h. m.
1	0·723	13·4	10·85	15·0	8·7	8·7	1·54	76°	3 40 after breakfast.
2	0·810	16·4	13·28	16·4	7·8	13·7	1·20	77	4 35 „
3	0·906	16·6	13·44	14·8	9·5	9·2	1·80	78	0 45 after luncheon.
4	0·801	15·6	12·63	15·8	9·7	11·0	1·42	73·5	3 15 „
5	0·654	13·1	10·61	16·2	10·0	13·0	1·01	69	3 45 „
6	0·543	10·7	8·66	15·9	12·7	17·0	0·63	67	4 15 „
7	0·652	11·7	9·47	14·5	15·3	12·9	0·91	65	4 45 „
Means	0·727	13·9	11·28	15·5	10·5	12·2	1·22	72·2	3 34 after a meal.

Experiments at the Summit of the Col du Géant, on myself.

Barometer 20·091 inches=515·6 millims. Altitude 11,030 feet (Ball)=3,362 metres. Fine day, a cold N.E. breeze.

	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced.	Air expired reduced for 1 grm. CO_2.	Number of expirations per minute without face-piece.	Number of expirations per minute into bag.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
July 18.								Fah.	h. m.
1	0·388	7·90	5·21	13·4	18	14·5	0·54	45°	2 55 after dinner. (lunch).
2	0·405	9·15	6·04	14·9	15·7	15·8	0·58	44	3 35 "
3	0·560	10·37	6·84	12·2	17·3	14·3	0·73	42	1 5 after tea.
4	0·466	8·63	5·70	12·2	12·7	12·0	0·72	41	1 35 "
5	0·415	9·36	6·18	14·9	13·3	11·8	0·79	41	2 10 "
6	0·450	8·57	5·66	12·6	15	11·0	0·78	41	2 50 "
July 19.									coffee,milk,bread,6.30 a.m.
7	0·401	7·87	5·19	12·9	14·3	14·2	0·55	38	0 45 after breakfast.
8	0·381	7·61	5·02	13·2	17·3	13·8	0·55	39	1 25 "
9	0·350	8·19	5·41	15·5	16·7	16·0	0·51	39	2 0 "
									food at 9.30 a.m.
10	0·419	8·09	5·34	12·8	17·0	14·2	0·57	46	0 45 after food.
11	0·476	9·44	6·23	13·1	14·7	13·8	0·68	42	2 20 "
12	0·507	10·58	6·98	13·8	14·7	15·5	0·68	42	3 5 "
Means	0·435	8·81	5·82	13·5	15·6	13·9	0·64	41·7	2 5 after food.

Experiments at the Summit of the Col du Géant, on M. David.

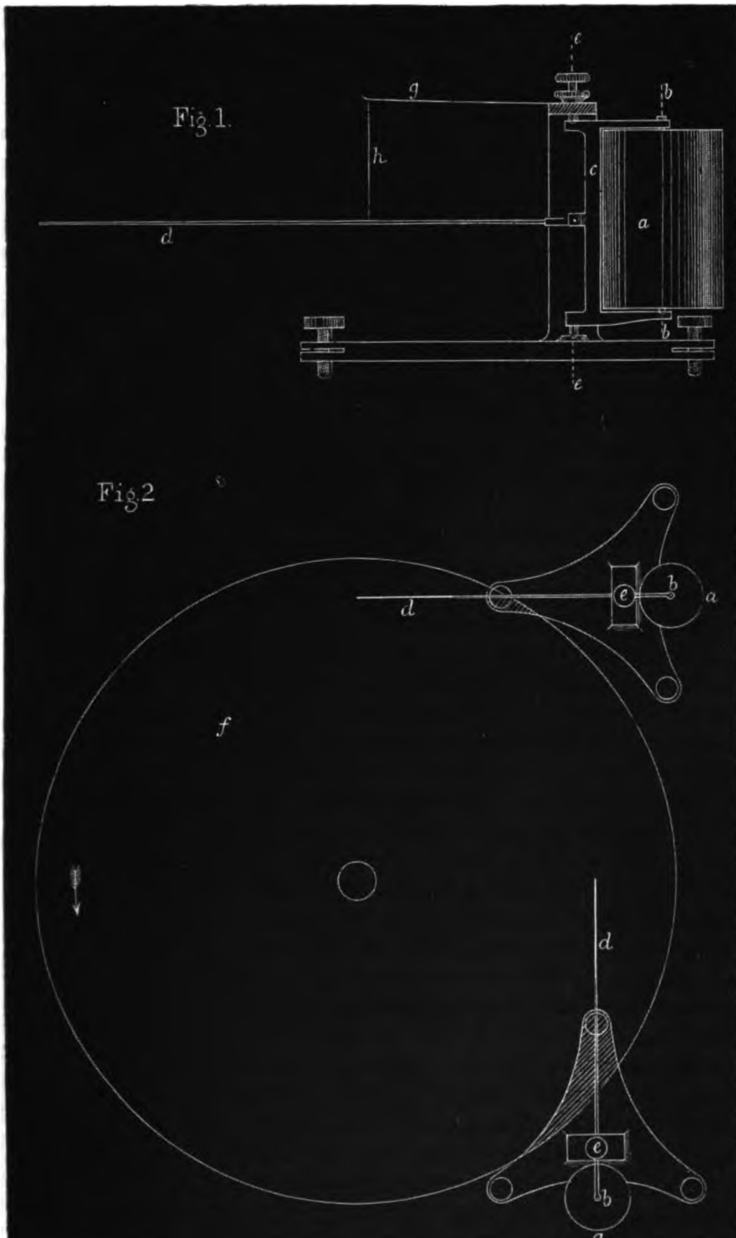
	Grm. CO_2 expired per minute.	Air expired per minute not reduced.	Air expired per minute reduced.	Air expired reduced for 1 grm. CO_2.	Number of expirations per minute without face-piece.	Number of expirations per minute into bag.	Volume air per expiration not reduced.	Temperature of air.	Time with reference to food taken.
								Fah.	h. m.
July 17.									Tea over 5. 30 p.m.
1	0·627	12·2	8·01	12·9	19·7	30·0	0·41	42°	0 20 after tea.
2	0·588	10·5	6·89	12·4	20·0	20·4	0·51	38 5	1 0 "
3	0·547	10·1	6·63	12·1	16·3	21·6	0·47	42	1 40 "
4	0·528	10·5	6·89	13·0	25·0	23·8	0·44	41	2 15 "
July 18.									
5	0·679	12·6	8·27	11·9	20·8	25·7	0·49	42	1 3 after breakfast.
6	0·692	13·3	8·73	12·6	22·0	28·3	0·47	40	1 47 "
7	0·597	12·3	8·07	13·5	18·7	26·0	0·48	45	2 33 "
8	0·519	10·6	6·96	13·4	20·7	25·4	0·42	46	3 25 "
9	0·616	11·7	7·68	12·5	21·7	24·4	0·48	47	1 0 after dinner.
10	0·665	12·5	8·20	12·3	21·0	24·0	0·52	47	1 30 "
11	0·752	14·0	9·19	12·2	20·7	25·2	0·56	46	1 55 "
12	0·532	10·9	6·56	12·3	20·0	18·2	0·55	44	3 45 after food.
Means	0·609	11·7	7·67	12·6	20·6	24·4	0·48	43·4	1 51 after food.

III. "On a New Seismograph." By J. A. Ewing, B.Sc., F.R.S.E., Professor of Mechanical Engineering in the University of Tokio, Japan. Communicated by Sir William Thomson, F.R.S. Received January 17, 1881.

The difficulty in earthquake measurements is to find a point which does not move during the disturbance. This condition is, in certain cases, fulfilled approximately (as regards horizontal motion) by the bob of a pendulum,* and, for this reason, pendulums have been frequently used as seismometers. A long pendulum, whose period greatly exceeds the period of the earthquake waves, suspended from an exceedingly rigid frame, makes a fairly good seismograph, the earth's motion relatively to the bob being shown by a pair of indicating levers at right angles to each other, with their short ends in contact with the bob near its centre of gravity, their fulcrums fixed to the earth, and their long ends lightly touching a plate of smoked glass which is kept revolving uniformly by clockwork. If the bob were to remain quite stationary during an earthquake, this would give two curves, showing on a magnified scale two rectangular components of the horizontal displacement of a point on the earth's surface in conjunction with the time, from which the amount and direction of the actual horizontal movement, and its velocity and rate of acceleration at any time during the shock, could be deduced. There are, however, two independent reasons why the bob of a pendulum seismograph does not remain stationary during an earthquake shock. In the first place, it is disturbed by the friction and inertia of the recording levers. [It may be observed here that a considerable multiplication of the motion is essential, at least in measurements of the small earthquakes which are common in Japan, for the actual motion of the earth's surface is usually only a fraction of a millimetre.] It is not difficult, however, to reduce this source of disturbance to an almost insensible quantity by a careful construction and arrangement of the joints, &c., and by making the mass of the bob excessively great relatively to that of the levers. The other and by far the more important cause of disturbance is the motion of the point of suspension of the pendulum. The motion communicated to the bob by this means would reach a maximum, and altogether eclipse the true earthquake motion if the period of oscillation of the supporting point agreed exactly with the pendulum's free period. By making the pendulum long enough, and the supporting frame rigid, this extreme case can easily be avoided, but the effect is never entirely absent. The writer has observed that

* And as regards vertical motion also if the bob be suspended by a spiral spring.

Fig. 1.



a pendulum 20 feet long, suspended as favourably to steadiness as possible, has been set swinging during even a very slight earthquake, through arcs about three times as long as the actual motion of the earth's surface. By using a pendulum of as great length as this, however, and by making it trace a continuous record in the manner described above, we may distinguish between that part of the record which is due to the swing of the pendulum and that part which is the true register of the earth's motion, the waves produced by the swing of the pendulum being of much longer periods than those due to the undulation of the earth. The record thus shows the short earthquake waves superposed upon a long sinuous curve due to the swing of the pendulum.

In this way, a long pendulum seismograph may (though with difficulty) be made to give good results; and one designed by the author has been erected in the University of Tokio, thanks to the liberality of the Japanese directors. More recently, however, the writer has designed and constructed another seismograph, which he believes to be novel, and which is certainly far superior to the long pendulum seismograph in simplicity and cheapness of construction, ease of use, and accuracy of results. To describe this new instrument is the object of the present paper.

Figs. 1 and 2 show as much of the apparatus as is needed for the purpose of explanation. The scale in fig. 1 is about one-third, and in fig. 2 about one-sixth, of full size.

A solid round cylindrical brass bob *a* is pivotted between two conical points of steel, so as to be capable of revolving freely about a vertical axis *bb* at the short end of a light horizontal lever. The horizontal lever consists of two parts—a light brass frame *c* and a long pointer of straw *d*, the extreme end of which is sharp-pointed, and presses gently against a revolving glass plate *f* (fig. 2), whose surface is smoked. The lever *cd* is pivotted about a vertical axis *ee*, between two conical steel points, the lower one working in a hole in the base; the upper one, the point of a set-screw fixed in the top of an inverted stirrup, which is in one piece with the base of the instrument. In fig. 1, one side of this stirrup is, for the sake of clearness, supposed to be removed. The base stands, supported by three levelling screws, on the top of a post, which is firmly driven into the ground, and then cut off short a few inches above the surface. The instrument is fixed to the post by a "geometrical clamp" (see Thomson and Tait's "Nat. Phil.," 2nd edition, vol. i, § 198). The sockets for the feet of the levelling screws are a pyramidal hole, a V slot, and a plane surface; and the pressure necessary to give steadiness is produced by a single screw bolt near the centre, which is not shown in the drawing. By this arrangement the axis *ee* is placed and held in a vertical position. It moves with the earth during a shock. There are two precisely

similar levers separately mounted at right angles to each other, and their pointers touch the plate at different distances from the centre (see Plan, fig. 2). The plate *f* revolves continuously in the direction of the arrow about a vertical axis, which is also fixed to the earth. It has not been considered necessary to show the details of this part of the apparatus.

The lever *cd* is proportioned so as to have this peculiarity, that the axes *ee* and *bb* are axis of percussion and axis of instantaneous rotation respectively. Hence, if a small horizontal displacement of the earth's surface takes place in a direction at right angles to *d*, carrying the axis *ee* with it, the lever will revolve through a small angle with *bb* as its instantaneous axis (if we neglect the effect of friction). There is, therefore, no tendency on the part of the bob *a* to acquire motion, either of translation or of rotation, and it remains at rest during and after the displacement. The same is true of any number of successive displacements in alternate directions.

The distance of the end of the pointer *d* from the axis *bb* is seven times as great as the distance of *ee* from *bb*, and hence the pointer moves through a distance seven times as great as the earth's motion. But the glass plate has moved in the same direction by an amount equal to the earth's motion. The recorded movement, drawn upon the plate in the direction of the radius, will therefore be six times the actual movement of the surface of the earth.

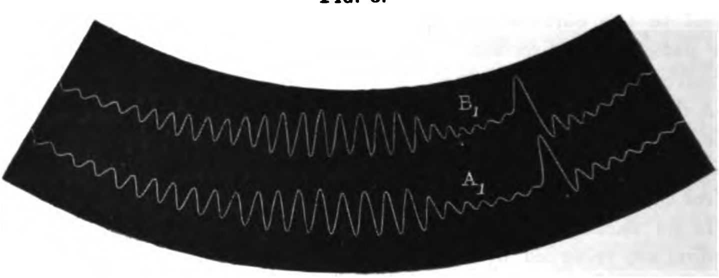
A displacement occurring at right angles to the former, or along the line *d*, produces no record by the one lever, for pointer and plate both move in the same direction and by the same amount. But this will affect the second lever, which is set at right angles to the first.

In an earthquake two rectangular components of the horizontal motion are recorded by the levers to which they are respectively perpendicular, and they are magnified in the same ratio. The revolution of the plate gives two continuous curves showing the successive displacements in conjunction with the time. When a displacement occurs, the pointer which has been displaced has no tendency either to return or to continue moving in the same direction, until the return displacement occurs, when it of course comes back. There is no directive force to produce return motion; and as soon as the axis *ee* stops moving, the lever *cd* comes to rest also. On account of this characteristic the new instrument has been called the *Astatic* Horizontal Lever Seismograph.

The ideal action of the instrument is interfered with to a small extent by friction. The friction between the marking point and the plate *f*, and at the pivots *ee*, prevents the axis *bb* from being the true instantaneous axis of the lever, and therefore there is a small amount of motion of translation communicated to the bob *a*. Again, the friction at the pivots *bb* tends to communicate motion of rotation to *a*.

But the actual amount of frictional error, in the first somewhat roughly made instrument which has been set up, is so small as hardly to affect the records to a sensible extent. To test it the following experiment was made :—The whole apparatus, consisting of the plate *f*, the clockwork to drive it, and the two levers, was put on a shaky table, whose movements could be made to imitate those of an earthquake. The pointers were placed side by side instead of in their usual positions, and the bob of one of them was held fast by a bracket from a neighbouring wall. It was, therefore, perfectly steady during the artificial earthquakes made by shaking the table. Two records were then obtained upon the plate, by shaking the table transversely to the levers: one was the true motion, known to be true because the bob of that lever was held fixed during the test; the other was the motion as recorded by the other lever in the usual way. Both motions were magnified in the same ratio of six to one. If the bob of the second lever remained stationary (as it should have done except for friction), the two records would have agreed exactly, and if not, then their difference would show the extent and nature of the frictional error.

Fig. 3.



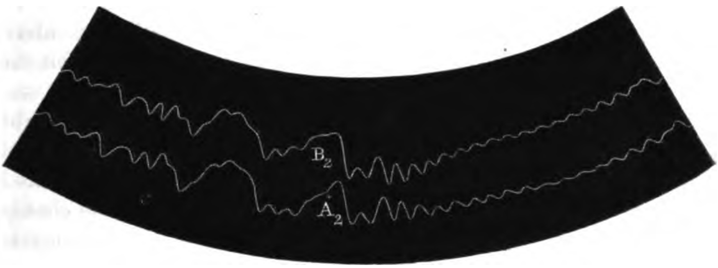
A_1 A_2 records produced by lever with fixed bob.
B_1 B_2 records produced by lever with free bob.

The result was satisfactory beyond expectation. Figs. 3 and 4, which are taken from a photograph of the plate, show the portions of the pair of records produced by this method. The curves A_1 in fig. 3, and A_2 in fig. 4, show the true motions as recorded by the lever whose bob was held fixed from outside. B_1 and B_2 are the corresponding records given by the other lever whose bob was free. It will be observed that the agreement is very close. Recent observations have shown that the parts of the curves in figs. 3 and 4, which are of relatively small amplitude, resemble most closely the curves produced by actual earthquakes. In these parts the divergence between the two records is scarcely perceptible.

The friction of the marking points on the plate gave some trouble

at first. The difficulty of getting a truly plane plate and of setting it exactly perpendicular to its axis of rotation, made it needful to allow a small amount of up-and-down motion of the marking points during the revolution of the plate. This has been done by connecting the

FIG. 4.



straw pointer d to the other part of the lever c, by a hinge joint, which gives the pointer freedom to move in a vertical plane, but leaves it no freedom to move sideways relatively to the part c. The effect of this would be that half the weight of the straw would be borne by the glass plate, which would give a disadvantageously large amount of pressure at the marking point. To remedy this, a very flexible straight steel spring, g, has been added, which stands out from the top of the stirrup, and bears a portion of the weight of d through the tension of a fine silk fibre, h. This arrangement retains the necessary vertical play of the pointer, without allowing its pressure on the plate to be greater than is just sufficient to rub off the thin coating of lamp-black which receives the record. So long as no earthquake occurs, the pointers continue to trace out, over and over again, the same two circles on the plate.

Owing to the imperfection of the driving clockwork, it has been found to be desirable to add an appliance for marking time at short intervals on the plate during an earthquake disturbance. This is done by a short pendulum, which remains deflected from its normal position by the attraction of an electro-magnet, through which a current is permanently passing. A contact breaker, arranged to be broken by the shock, interrupts the current and releases the pendulum, whose beats then mark a scale of time upon the revolving plate during about one revolution, after which they cease to mark.

Since the seismograph was made it has recorded several earthquakes quite successfully. The first of these (on November 3, 1880) was found to consist of more than 150 successive undulations of irregular amplitudes and periods. The mean period of a complete wave was about 0·6 second, and the greatest total amplitude of the earth's

motion was 0·3 millim. Another earthquake (on November 10), which, judged by its shaking effects on houses, &c., seemed to be of much greater violence than the first, gave a record which showed that its greater intensity was due, not to greater extent of movement, but to greater frequency in the waves; for their amplitude was even somewhat smaller, but their frequency about three times greater than in the former case. There were, however, at the beginning of this earthquake a few waves of long period, upon which the above-mentioned short waves were superposed: after them the motion consisted of nothing but an irregular ripple of short waves.

These results show that a higher ratio of multiplication is desirable. The description which has been given applies, with insignificant modifications, to the instrument now in use at the University of Tokio. In making a second instrument the writer would introduce some changes by way of adding to the stability of the bob and diminishing the friction. Its mass and its moment of inertia might be increased without increasing the distance between the axes *bb* and *ee*, by making it in the form of a hollow ring, and placing the vertical part of the lever *c* inside. To reduce the friction connexion between the lever and its supports on the line *ee* might be given by pressure against horizontal friction wheels, two at the top and two at the bottom, the fifth degree of constraint being supplied by the pressure of a rounded steel foot against an agate plate in the base. Other modifications might be mentioned, but the author's object in this paper has been not so much to describe constructive details as to explain the principle on which the action of the new seismograph depends.

February 17, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Viscosity of Gases at High Exhaustions." By WILLIAM CROOKES, F.R.S. Received December 26, 1880.

(Abstract.)

By the viscosity or internal friction of a gas, is meant the resistance it offers to the gliding of one portion over another. In a paper read

before the British Association in 1859, Maxwell* presented the remarkable result that on theoretical grounds the coefficient of friction, or the viscosity, should be independent of the density of the gas, although at the same time he states that the only experiments he had met with on the subject did not seem to confirm his views.

An elaborate series of experiments were undertaken by Maxwell to test so remarkable a consequence of a mathematical theory; and in 1866, in the Bakerian Lecture for that year,† he published the results under the title of "The Viscosity or Internal Friction of Air and other Gases." He found the coefficient of friction in air to be practically constant for pressures between 30 inches and 0·5 inch; in fact numbers calculated on the hypothesis that the viscosity was independent of the density agreed very well with the observed values.

The apparatus used by Maxwell was not of a character to admit of experiments with much lower pressure than 0·5 inch.

Maxwell's theory, that the viscosity of a gas is independent of the density, presupposes that the mean length of path of the molecules between their collisions is very small compared with the dimensions of the apparatus; but inasmuch as the mean length of path increases directly with the expansion, whilst the distance between the molecules only increases with the cube root of the expansion, it is not difficult with the Sprengel pump to produce an exhaustion in which the mean free path is measured by inches, and even feet, and at exhaustions of this degree it is probable that Maxwell's law would not hold good.

The experiments recorded in this paper were commenced early in 1876, and have been continued to the present time. In November, 1876, the author gave a note to the Royal Society on some preliminary results. Several different forms of apparatus have since been used one after the other, with improvements and complexities suggested by experience or rendered possible by the extra skill acquired in manipulation. The earlier observations are now of little value, but the time spent in their prosecution was not thrown away, as out of those experiments has grown the very complicated apparatus now finally adopted.

The viscosity torsion apparatus with which all the experiments here given have been performed, is a very complicated instrument, and cannot be well understood without the accompanying drawings. It consists essentially of a glass bulb, blown with a point at the lower end, and sealed on to a long narrow glass tube. In the bulb is suspended a plate of mica, by means of a fine fibre of glass, 26 inches long, which is sealed to the top of the glass tube, and hangs vertically along its axis. The plate of mica is ignited and lamp-blacked over one-half. The tube is pointed at the upper end, the upper and lower points are 46 inches apart, and are accurately in the prolongation of

* "Phil. Mag.," 4th ser., vol. xix, p. 31.

† "Phil. Trans.," 1866, Part I, p. 249.

the axis of the tube. Sockets are firmly fixed to a solid support, so that when the tube and bulb are clamped between them they are only able to move around the vertical axis. The glass fibre being only connected with the tube at the top, rotating the tube on its axis communicates torsion to the fibre, and sets the mica plate swinging on the same axis without giving it any pendulous movement. The diameter of the fibre is about 0·001 inch. The viscosity apparatus is connected to the pump by a flexible glass spiral, so as to allow the apparatus to rotate on the pivots and at the same time to be connected to the pump altogether with sealed glass joints. An arm working between metal stops, limits the rotation to the small angle only which is necessary.

The torsional movement given to the mica plate, by the light of the candle shining on it or by the rotation of the bulb and tube on its axis by the movement of the arm between the stops, is measured by a beam of light from a lamp, reflected from a mirror to a graduated scale.

The pump employed has already been described. The measuring apparatus is similar to that described by Professor McLeod* before the Physical Society, June 13th, 1874. As it contains several improvements shown by experience to be necessary when working at very high vacua, a detailed description is given in the paper.

When taking an observation the arm is moved over to the stop, and in a few seconds allowed to return to its original position by the action of a spring. This movement rotates the viscosity apparatus through a small angle, and sets the mica plate vibrating, the reflected line of light traversing from one side of the scale to the other in arcs of diminishing amplitude till it finally settles down once more at *zero*.

The observer watching the moving index of light records the scale number reached at the extremity of each arc. The numbers being alternately on one and the other side of zero are added two by two together, to get the value of each oscillation. The logarithms of these values are then found and their differences taken; the mean of these differences is logarithmic decrement per swing of the arc of oscillation. For the sake of brevity this is called the log dec.

A very large number of experiments have been made on the viscosity of air and other gases. Observations have been taken at as high an exhaustion as 0·02 M, but at these high points they are not sufficiently concordant to be trustworthy. The pump will exhaust to this point without difficulty if a few precautions are taken, but at this low pressure the means of measuring fail in accuracy.

The precautions which experience shows to be necessary when exhausting to the highest points are fully described in the paper.

* "Phil. Mag.," vol. xlviii, p. 110, August, 1874.

Viscosity of Air.

The mean of a very large number of closely concordant results gives as the log decrement for air for the special apparatus employed, at a pressure of 760 millims. of mercury and a temperature of 15° C., the number 0·1124. According to Maxwell, the viscosity should remain constant until the rarefaction becomes so great that we are no longer at liberty to consider the mean free path of the molecules as practically insignificant in comparison with the dimensions of the vessel.

The author's observations show that this theoretical result of Maxwell's is at least approximately and may be accurately true in air up to such exhaustions as are above referred to; and that at higher exhaustions the viscosity falls off, as it might be expected to do according to theory.

The results are embodied in a table and diagrams.

The first half of the table gives the viscosity of air, in so far as it is represented by the log decrement, at pressures intermediate between 760 millims. and 0·76 millim. (1,000 millionths of an atmosphere). In order to avoid the inconvenience of frequent reference to small fractions of a millimetre, the millionth of an atmosphere* (=M) is now taken as the unit instead of the millimetre. The second half of the table is therefore given in millionths, going up to an exhaustion of 0·02 millionth of an atmosphere.

Starting from the log decrement 0·1124 at 0·760 millim., the viscosity diminishes very regularly but at a somewhat decreasing rate. Between 50 millims. and 3 millims. the direction is almost vertical, and a great change in the uniformity of the viscosity curve commences at a pressure of about 3 millims. At this point the previous approximation to, or coincidence with, Maxwell's law begins to fail, and further pumping considerably reduces the log decrement.

From 1,000 M the diminution of viscosity is very slight until the exhaustion reaches about 250 M; after that it gets less with increasing rapidity, and falls away quickly after 35 M is reached.

The curves of increasing mean free path and diminishing viscosity closely agree. This agreement is more than a mere coincidence, and is likely to throw much light on the cause of viscosity of gases.

In the table is also given the measurements of the repulsion exerted on the blackened end of the mica plate by a candle-flame placed 500 millims. off. The repulsion due to radiation commences just at about the same degree of exhaustion where the viscosity begins to decline rapidly, and it principally comes in at the exhaustions above 1,000 M.

The close agreement between the loss of viscosity and the increased action of radiation is very striking up to the 35 millionth, when the repulsion curve turns round and falls away as rapidly as the viscosity.

* 1 M = 0·00076 millim.; 1315·789 M = 1 millim.

Experiments are next described on the resistance of air to the passage of an induction spark.

Since the publication of the author's researches on the phenomena presented by the passage of the induction discharge through high vacua, the present results—which, although never published, precede by a year or two those just mentioned—have lost much of their interest.

The phenomena at the very high exhaustion of 0·02 M may be of interest. With a coil giving a spark 85 millims. long, no discharge whatever passes. On increasing the battery power till the striking distance in air was 100 millims. the spark occasionally passed through as an intermittent flash, bringing out faint green phosphorescence on the glass round the end of the — pole.

On one occasion the author obtained a much higher exhaustion than 0·02 M. It could not be measured, but from the repulsion by radiation and the low log decrement it was probably about 0·01 M. The terminals of the vacuum tube and wires leading to them were well insulated, and the full power of a coil giving a 20-inch spark was put on to it. At first nothing was to be seen. Then a brilliant green light flashed through the tube, getting more and more frequent. Suddenly a spark passed from a wire to the glass tube, and broke it, terminating the experiment.

Since these experiments, vacua have frequently been got as high, and even higher, but the author has never seen one that would long resist the 20-inch spark from his large coil.

Viscosity of Oxygen.

The series of experiments with air show a complete history of its behaviour between very wide limits of pressure. It became interesting to see how the two components of air, oxygen and nitrogen, would behave under similar circumstances. Experiments were therefore instituted exactly as in the case of dry air, but with the apparatus filled with pure oxygen.

The results are given in the form of tables and plotted as curves on diagrams.

The figures show a great similarity to the air curve. Like it the log decrement sinks somewhat rapidly between pressures from 760 millims. to about 75 millims. It then remains almost steady, not varying much till a pressure of 16 millims. is reached. Here, however, it turns in the opposite direction, and increases up to 1·5 millim. It then diminishes again, and at higher exhaustions it rapidly sinks. This increase of viscosity at pressures of a few millimetres has been observed in other gases, but only to so small an extent as to be scarcely beyond the limits of experimental error. In the case of

oxygen, however, the increase is too great to be entirely attributable to this cause.

Oxygen has more viscosity than any gas yet examined. The viscosity of air at 760 millims. being 0·1124, the proportion between that of air and oxygen, according to these results, is 1·1185.

This proportion of 1·1185 holds good (allowing for experimental errors) up to a pressure of about 20 millims. Between that point and 1 millim. variations occur, which have not been traced to any assignable cause: they seem large to be put down to "experimental errors." The discrepancies disappear again at an exhaustion of about 1 millim., and from that point to the highest hitherto reached the proportion of 1·1185 is fairly well maintained.

Viscosity of Nitrogen.

The proportion between the viscosities of nitrogen and air at a pressure of 760 millims. is, according to these experiments, 0·9715.

A comparison of the air curves with those given by oxygen and nitrogen gives some interesting results. The composition of the atmosphere is, by bulk,

Oxygen........................	20·8
Nitrogen	79·2
	100·0

The viscosity of the two gases is almost exactly in the same proportion: thus at 760 millims.—

$$\frac{20\cdot8 \text{ vis. O} + 79\cdot2 \text{ vis. N}}{100} = \text{vis. air},$$

$$\frac{20\cdot8(0\cdot1257) + 79\cdot2(0\cdot1092)}{100} = \quad ,,$$

$$\frac{2\cdot61456 + 8\cdot64072}{100} = 0\cdot11255,$$

a result closely coinciding with 0·1124, the experimental result for air. Up to an exhaustion of about 30 M the same proportion between the viscosities of air, oxygen, and nitrogen is preserved with but little variation. From that point divergence occurs between the individual curves of the three gases.

Observations on the spectrum of nitrogen are next given.

The curve of repulsion exerted by radiation is plotted on the diagrams. It is much lower than in oxygen or air, and sinks rapidly after the maximum is passed.

Viscosity of Carbonic Anhydride.

The curves of this gas are given in diagrams plotted from the

observations. At first the curve seems to follow the same direction as the air curve. But at a pressure of about 620 millims. it slopes more rapidly till the pressure is reduced to about 50 millims., when the curve again takes the direction of the air curve. The total diminution between 760 millims. and 1 millim. is nearly double that of air.

The proportion between the viscosity of carbonic anhydride and air at 760 millims. is 0·9208.

Viscosity of Carbonic Oxide.

The results with this gas are remarkable as showing an almost complete identity with those of nitrogen both in position and shape. The viscosity at 760 millims. is in each case 0·1092.

Like that of nitrogen, the curve of carbonic oxide is seen to be vertical, *i.e.*, assuming the curve to represent the viscosity, the gas obeys Maxwell's law, at pressures between 90 millims. and 3 millims. The straight portion in nitrogen is at a little higher pressure,—between 100 millims. and 6 millims.

The curve of repulsion resulting from radiation is lower in carbonic oxide than in any other gas examined, and, unlike the other gases, there is no sudden rise to a maximum at about 40 M. At lower exhaustions the curve is, however, higher than it is in nitrogen.

Viscosity of Hydrogen.

It has been found that hydrogen has much less viscosity than any other gas; the fact of the log decrement not decreasing by additional attempts at purification is the test of its being free from admixture. This method of ascertaining the purity of the gas, by the uniformity of its viscosity coefficient at 760 millims., is more accurate than collecting samples and analysing them eudiometrically.

Several series of observations in hydrogen have been taken. For a long time it was considered that hydrogen, like other gases, showed the same slight departure from Maxwell's law of viscosity being independent of density that appeared to be indicated with other gases; for the log decrement persistently diminished as the exhaustion increased, even at such moderate pressures as could be measured by the barometer gauge. Had it not been that the rate of decrease was not uniform in the different series of observations, it might have been considered that this variation from Maxwell's law was due to some inherent property of all gases. After working at the subject for more than a year, it was discovered that the discrepancy arose from a trace of water obstinately held by the hydrogen. Since discovering this property, extra precautions (already described at the commencement of this paper) have been taken to dry all gases before entering the apparatus.

The remarkable character of hydrogen is the uniformity of resistance which it presents. It obeys Maxwell's law almost absolutely up to an exhaustion of about 700 M, and then it commences to break down. Up to this point the line of viscosity is almost vertical. It then commences to curve over, and when the mean free path assumes proportions comparable with the dimensions of the bulb, and approaches infinity, the viscosity curve in like manner draws near the zero line.

The repulsive force of radiation is higher in hydrogen than in any other gas. It commences at as low an exhaustion as 14 millims., but does not increase to any great extent till an exhaustion of 200 M is attained; it then rises rapidly to a maximum at between 40 and 60 M, after which it falls away to zero. The maximum repulsion exerted by radiation in hydrogen is to that in air as 70 to 42·6. This fact is now utilised in the construction of radiometers and similar instruments when great sensitiveness is required.

Taking the viscosity of air at 760 millims. as 0·1124, and hydrogen as 0·0499, the proportion between them is 0·4439.

The Spectrum of Hydrogen.

The red line ($\lambda=6562$), the green line ($\lambda=4861$), and the blue line ($\lambda=4340$) are seen at their brightest at a pressure of about 3 millims., and after that exhaustion they begin to diminish in intensity. As exhaustion proceeds, a variation in visibility of the three lines is observed. Thus at 36 millims. the red line is seen brightly, the green faintly, whilst the blue line cannot be detected. At 15 millims. the blue line is seen, and the three keep visible till an exhaustion of 418 M is reached, when the blue line becomes difficult to see. At 38 M only the red and green lines are visible, the red being very faint. It is seen with increasing difficulty up to an exhaustion of 2 M, when it can be seen no longer. The green line now remains visible up to an exhaustion of 0·37 M, beyond which it has not been seen.

It is worthy of remark that although, when working with pure hydrogen, the green line is always the last to go, it is not the first to appear when hydrogen is present as an impurity in other gases. Thus when working with carbonic anhydride insufficiently purified, the red hydrogen line is often seen, but never the green or the blue line.

Influence of Aqueous Vapour on the Viscosity of Air.

In the foregoing experiments many discrepancies were traced to the presence of moisture in the gas. The influence of aqueous vapour does not appear to be great when present in moderate amount in gas of normal density, but at high exhaustions it introduces errors which interfere with the uniformity of the results. A series of experiments were accordingly undertaken to trace the special action of aqueous vapour when mixed with air.

Up to a pressure of about 350 millims. the presence of aqueous vapour has little or no influence on the viscosity of air. The two curves are, in fact, superimposed. At this point, however, divergence commences, and the curve rapidly bends over, the viscosity falling from 0·0903 to 0·0500, between 50 and 7 millims. pressure. Here it joins the hydrogen curve, and between 7 millims. and 1 millim. they appear to be identical.

These results are partly to be explained by the peculiar action of water vapour in the apparatus. At the normal pressure the amount of aqueous vapour present in the air, supposing it to be saturated, is only about 13 parts in a million, and the identity of the log decrement with that of dry air shows that this small quantity of water has no appreciable action on the viscosity. When the pump is set to work the air is gradually removed, whilst the aqueous vapour is kept supplied from the reservoir of liquid. As the exhaustion approaches the tension of aqueous vapour, evaporation goes on at a greater rate, and the vapour displaces the air with increasing rapidity; until, after the pressure of 12·7 millims. is passed, the aqueous vapour acts as a gas, and, being constantly supplied from the reservoir of water (as long as it lasts), washes out all the air from the apparatus, the log decrement rapidly sinking to that of pure water gas.

This explanation requires that the viscosity of pure aqueous vapour should be the same as that of hydrogen, at all events between 7 millims. and 1 millim. pressure. The facts can, however, be explained in another way. During the action of the Sprengel pump sufficient electricity is some times generated to render the fall tubes luminous in the dark. It is conceivable that under such electrical influence the falling mercury may be able to decompose aqueous vapour at these high exhaustions, with formation of oxide of mercury and liberation of hydrogen. Of these two theories the latter appears to be the more probable.

The presence of water vapour shows itself likewise in the very slight amount of repulsion produced by radiation. Repulsion commences in air at a pressure of 12 millims., whilst at a higher exhaustion the maximum effect rises to over 40 divisions. Here, however, repulsion does not begin till the exhaustion is higher than the barometer gauge will indicate, whilst the maximum action after long-continued pumping is only 9 divisions.

Viscosity of Kerosoline Vapour.

The rapid diminution of viscosity in the last experiment after reaching the pressure of 400 millims. is probably due to the aqueous vapour in the air being near its liquefying point. It was thought advisable to test this hypothesis by employing a somewhat less easily condensible vapour, which could be introduced into the apparatus without

any admixture of air. An experiment was accordingly tried with a very volatile hydrocarbon, commercially known as kerosoline, boiling at a little above the ordinary temperature. The vapour of this body was introduced into the well-exhausted apparatus, when the gauge at once sank 82·5 millims. After the usual precautions to eliminate air a series of observations were taken.

The loss of viscosity is more rapid than with any other gas examined except aqueous vapour. Conversely a very great increase of viscosity occurs on increasing the pressure from 8 to 82·5 millims. The explanation of this is that the vapour of kerosoline is very near its liquefying point, and therefore very far from the state of a "perfect" gas.

The negative bend in the curve at about 10 millims. pressure, already noticed with other gases, is strongly marked with this hydrocarbon vapour.

Discussion of Results.

When discussing the viscosity results obtained with the different gases experimented with, the author gives the following approximate comparison of viscosities, such as is afforded by a comparison of the log decrements of each gas and that of air, comparing the ratio with that obtained by Graham, Kundt and Warburg, and Maxwell.

	Graham.	Kundt and Warburg.	Maxwell.	Crookes.
Air	1·0000	1·0000	1·0000	1·0000
Oxygen	1·1099	..	..	1·1185
Nitrogen	0·971	..	..	0·9715
Carbonic oxide	0·971	..	..	0·9715
Carbonic anhydride	0·807	0·806	0·859	0·9208
Hydrogen	0·4855	0·488	0·5156	0·4439

Graham's numbers are the theoretical results deduced from his experiments on transpiration of gases. They are, he says,* the numbers to which the transpiration times of the gases approximate and in which they have their limit. Graham concludes that the "times of oxygen, nitrogen, carbonic oxide, and air are directly as their densities, or equal *weights* of these gases pass in equal times. Hydrogen passes in half the time of nitrogen, or twice as rapidly for equal volumes. The result for carbonic acid appears at first anomalous. It is that the transpiration time of this gas is inversely proportional to its density when compared with oxygen."

The proportion between air and oxygen, nitrogen, or carbonic oxide is not very different at any degree of exhaustion to that which it is at

* *Loc. cit.*, pp. 178, 179.

760 millims. Carbonic anhydride, however, is different; the proportion between it and air holds good between 760 and 650 millims. Then it gets lower and lower as the pressure sinks, until 50 or 55 millims. is reached, when the proportion between it and air becomes constant.

Hydrogen, however, is entirely different to the other gases; its log decrement remains the same to a very high exhaustion, and, that of other gases sinking, it is evident that the proportion between this gas and any other is different for each pressure.

It must not be forgotten that the pressure of 760 millims. is not one of the constants of Nature, but is a purely arbitrary one, selected for our own convenience when working near the level of the sea. In the diagrams accompanying the paper the author has started from this pressure of 760 millims., and has given the log decrement curves which approximately represent the viscosities through a wide range of exhaustion. But the curves might also be continued, working downwards instead of upwards. From the shape and direction in which they cut the 760 line it is reasonable to infer their further progress downwards, and we may assume that an easily liquefiable gas will show a more rapid increase in viscosity than one which is difficult to liquefy by pressure. For instance, hydrogen, the least condensible of all gases, shows no tendency to increase in log decrement by pressure. Oxygen and nitrogen, which are only a little less difficult to condense than hydrogen, show a slight increase in log decrement. Carbonic anhydride, which liquefies at a pressure of 56 atmospheres at 15° C., increases so rapidly in log decrement that at this pressure it would have a log decrement of about 1·3, representing an amount of resistance to motion that it is difficult to conceive anything of the nature of gas being capable of exerting.

Kerosoline vapour is rendered liquid by pressure much more readily than carbonic anhydride. Its curve shows a great increase in density for a very slight access of pressure.

Again, aqueous vapour is condensible to the liquid form with the greatest readiness; and the almost horizontal direction of the curve representing aqueous vapour mixed with air carries out the hypothesis.

It follows, then, that Maxwell's law holds good for perfect gases. The disturbing influence spoken of in the commencement of this paper as occasioning a variation from Maxwell's law, is the tendency to liquefaction, which prevents us from speaking of any gas as "perfect," and which hinders it from obeying Boyle and Mariotte's law. The nearer a gas obeys this law the more closely does it conform to Maxwell's law.

Maxwell's law was discovered as the consequence of a mathematical theory. It presupposes the existence of gas in a "perfect" state—a state practically unknown to physicists, although hydrogen gas very nearly approaches that state. An ordinary gas may be said to be

bounded, as regards its physical state, on the one side by the sub-gaseous or liquid condition, and on the other side by the ultra-gaseous condition. A gas assumes the former state when condensed by pressure or cold, and it changes to the latter state when highly rarefied. Before actually assuming either of these states there is a kind of foreshadowing of change, with partial loss of gaseity. When the molecules, by pressure or cold, are made to approach each other more closely, they begin to enter the sphere of each other's attraction, and therefore the amount of pressure or cold necessary to produce a certain density is less than the theoretical amount by the internal attraction exerted on each other by the molecules. The nearer the gas approaches the point of liquefaction the greater is the attraction of one molecule to another, and the amount of pressure required to produce any given density will be proportionally less than that theoretically required by a "perfect" gas.

The Ultra-gaseous State of Matter.

After some theoretical considerations respecting the viscosity of gases, the author concludes with the detailed statement of his theory of the existence of an ultra-gaseous state of matter.

A consideration of the curves of the gases, especially hydrogen, which are given in the paper, will confirm the supposition that a gas, as the exhaustions become extreme, gradually loses its gaseous characteristics, and passes to an ultra-gaseous state.

An objection has been raised touching the existence of ultra-gaseous matter in highly exhausted electrical tubes, that the special phenomena of radiation and phosphorescence which the author has considered characteristic of this form of matter can be made to occur at much lower pressures than that which exhibits the maximum effects. For the sake of argument let us assume that the state of ultra gas with its associated phenomena is at the maximum at a millionth of an atmosphere. Here the mean free path is about 4 inches long, sufficient to strike across the exhausted tube. But it has been shown by many experimentalists that at exhaustions so low that the contents of the tube are certainly not in the ultra-gaseous state, the phenomena of phosphorescence can be observed. This circumstance had not escaped the author's notice. In his first paper on the "Illumination of Lines of Molecular Pressure and the Trajectory of Molecules,"* the author drew attention to the fact that a molecular ray producing green phosphorescence can be projected 102 millims. from the negative pole when the pressure is as high as 0·324 millim., or 427 M. In this case the mean free path of the molecules is 0·23 millim.; and it is not surprising that with more powerful induction discharges, and with

* "Phil. Trans.," Part I, 1879. The Bakerian Lecture.

special appliances for exalting the faint action to be detected, the above-named phenomena can be produced at still higher pressures.

It must be remembered that we know nothing of the *absolute* length of the free path or the *absolute* velocity of a molecule; these may vary almost from zero to infinity. We must limit ourselves to the *mean* free path and the *mean* velocity, and all that these experiments show is that a few molecules can travel more than a hundred times the *mean* free path, and with perhaps a corresponding increase over the *mean* velocity, before they are stopped by collisions. With weak electrical power the special phosphorogenic action of these few molecules is too faint to be noticed; but by intensifying the discharge the action of the molecules can be so increased as to render their presence visible. It is also probable that the absolute velocity of the molecules is increased so as to make the mean velocity with which they leave the negative pole greater than that of ordinary gaseous molecules. This being the case, they will not easily be stopped or deflected by collisions, but will drive through obstacles, and so travel to a greater distance.

If this view is correct, it does not follow that gas and ultra gas can coexist in the same vessel. All that can be legitimately inferred is, that the two states insensibly merge one into the other, so that at an intermediate point we can by appropriate means exalt either the phenomena due to gas or to ultra gas. The same thing occurs between the states of solid and liquid, and liquid and gas. Tresca's experiments on the flow of solids prove that lead and even iron, at the common temperature, possess properties which strictly appertain to liquids, whilst Andrews has shown that liquid and gas may be made to merge gradually one into the other, so that at an intermediate point the substance partakes of the properties of both states.

"Note on the Reduction of Mr. Crookes's Experiments on the Decrement of the Arc of Vibration of a Mica Plate Oscillating within a Bulb containing more or less rarefied Gas." By Professor G. G. STOKES, Sec. R.S. Received February 17, 1881.

(Abstract.)

The determination of the motion of the gas within the bulb, which would theoretically lead to a determination of the coefficient of viscosity of the gas, forms a mathematical problem of hopeless difficulty. Nevertheless we are able, by attending to the condition of similarity of the motion in different cases, to compare the viscosities of the different gases for as many groups of corresponding pressures as we please. Setting aside certain minute corrections, which would have vanished

altogether had the moment of inertia of the vibrating body been sufficient to make the time of vibration sensibly independent of the gas, as was approximately the case, the condition of similarity is that the densities shall be as the log decrements of the arc of vibration, and the conclusion from theory is that when that condition is satisfied, then the viscosities are in the same ratio. Pressures which satisfy the condition of similarity are said to "correspond."

It was found that on omitting the high exhaustions, the experiments led to the following law:—

The ratios of the viscosities of the different gases are the same for any two groups of corresponding pressures. In other words, if the ratios of the viscosities of a set of gases are found (they are given by the ratios of the log decrements) for one set of corresponding pressures, these pressures may be changed in any given ratio without disturbing the ratios of the viscosities.

This law follows of course at once from Maxwell's law, according to which the viscosity of a gas is independent of the pressure. It does not, however, by itself alone prove Maxwell's law, and might be satisfied even were Maxwell's law not true. The constancy, however, of the log decrement, when the circumstances are such that the molar inertia of the gas may presumably be neglected, proves that at any rate when the density is not too great that law is true; and the variability of the log decrement at the higher pressures in all but the very light gas hydrogen is in no way opposed to it, though Mr. Crookes's experiments do not enable us to test it directly, but merely establish a more general law, which embraces Maxwell's as a particular case.

The viscosities referred to air as unity which came out from Mr. Crookes's experiments were as follows:—

Oxygen	1·117
Nitrogen and carbonic oxide	0·970
Carbonic anhydride	0·823
Hydrogen	0·500

The viscosity of kerosoline vapour could not be accurately deduced from the experiments, as the substance is a mixture, and the vapour density therefore unknown. Assuming the relative viscosity to be 0·0380, the vapour density required to make the experiments fit came out 3·408 referred to air, or 49·16 referred to hydrogen.

When once the density is sufficiently small, the log decrement may be taken as a measure of the viscosity. Mr. Crookes's tables show how completely Maxwell's law breaks down at the high exhaustions, as Maxwell himself foresaw must be the case. Not only so, but if we take pressures at those high exhaustions which are in the same ratios as "corresponding" pressures, the log decrements in the different gases are by no means in the ratios of the densities.

It would appear as if the mechanical properties of a gas at ordinary pressures and up to extreme exhaustions (setting aside the minute deviations from Boyle's law, &c.), were completely defined by two constants, suppose the density at a given pressure and the coefficient of viscosity, but at the high exhaustions at which phenomena of "ultra-gas" begin to appear, specific differences came in, to include which an additional constant, or perhaps more than one, requires to be known.

II. "Notes on the Earthquakes of July, 1880, at Manila." By Commander W. B. PAULI, R.N., Her Britannic Majesty's Consul at Manila. Received January 18, 1881.

The following "Notes" have been communicated from the Foreign Office, by direction of Earl Granville, K.G., F.R.S.

I was unfortunately obliged to leave Manila last June on sick leave, and the news of the late disastrous earthquakes reached Europe shortly after my arrival.

My knowledge of the locality enabled me to realise the extent of the calamity even with the bare details given by the telegrams.

I have collected the fullest information obtainable at this distance, chiefly from local papers and letters from friends, but by far the most important data I have received are contained in the scientific observations sent me by Father Faura, the Director of the Municipal Observatory at Manila, who being a pupil of the late Father Secchi, of Rome, and himself taking keen interest in science, chiefly in relation to storms and earthquakes, is a particularly fit person for his present post.

The Manila Observatory is now furnished with seismographical instruments, by which the accompanying diagrams of the chief shocks were obtained.

These, with the translation of Father Faura's observations on them, I trust may be of interest, especially as this is the first scientific account of earthquakes in the Philippine Islands, where they are so frequent and so violent.

To this account I have appended further information derived from other sources respecting the course and extent of the disturbances, but have purposely omitted the details of the personal experiences of individuals and the loss sustained in life and property, confining my remarks to such accounts as bear directly on the phenomena in a scientific point of view.

A short preliminary account of the volcanic systems of Luzon, the seat of the late earthquakes, may prove a useful introduction.

Don José Centeno y Garcia, Inspector-General of Mines to the

Philippine Government, in a work called "Memoria Geologico Minera de los Islas Filipinas," observes:—"That the volcanic action in the Philippine Archipelago is not displayed in an arbitrary or irregular manner, but, on the contrary, is exercised by defined lines almost parallel to each other, whose direction may be fixed as from N.N.W. to S.S.E., thus showing two parallel systems, which to distinguish from each other we will call the system of Taal, and that of Mayon, being the names of the two volcanoes of the greatest and most frequent activity in the two systems."

The first, viz., the system of Taal, commences in the north of Luzon, and after traversing the greater part of the island, including in its course the active volcano of Taal, is lost in the sea of Mindoro, to reappear in other islands of the Archipelago.

The second system, "of Mayon," is situated to the eastward of the first, and is represented by the magnificent active volcano of Mayon of 8,000 feet altitude, and although parallel to the first, does not extend farther to the north than the extinct volcano of Isero, in the province of South Camerines, in Luzon, but traverses the southern portion of the island, and includes the active volcano of Bulasan, at the extreme southern point of Luzon; it appears again in other islands, especially in the large island of Mindanoa.

Señor Centeno observes that these two distinctly parallel lines in Luzon, with a distance of about 120 miles between them, after leaving that island, approach each other so as to be only 64 miles apart in Mindanoa, and in his opinion join each other in the southern group of the Mollucas.

The present earthquakes extended from Vigan, in the north, to Tayabas in the south, being in the line of the system of Taal, which volcano showed considerable and unusual activity during the disturbance. The volcano of Mayon, on the contrary, is reported as being in its normal state, emitting no more than its usual amount of smoke, although the volcano of Bulusan (in the system of Mayon, and far to the southward of that mountain), which seldom shows signs of activity, had recommenced to throw up smoke. This would appear to corroborate Señor Centeno's theory, that the two systems are distinct in Luzon, but join each other far to the southward; and that the effects of the earthquakes on volcanoes was confined to the system of Taal on this occasion in Luzon, extending to the point where the two systems united in the south, and penetrating on its return to the north, along the system of Mayon, as far only as Bulusan, but not reaching so far north as Mayon.

The extent of these earthquakes is said to have been from Vigan to Tayabas, a distance of about 220 nautical miles in length, with an average width of about 75 miles. No shocks are reported from the extreme north or south of Luzon.

Summary of the Seismometrical Observations taken at the Municipal Observatory of Manila, during the various earthquakes which took place from the 14*th to the* 25*th July*, 1880.

The observations commence by a description of the instruments employed to trace the figures representing the undulations; Father Faura continues:—"The object of both apparatuses is first to discover the direction of the first horizontal undulation, which is obtained by means of the small ring at the end of the pendulum, and forced along by it.

"2nd. To ascertain the general direction of the horizontal undulations, and their amplitude by means of the lines which the same pendulum leaves in the powder.

"3rd. To obtain the greatest amplitude of the greatest vertical undulation by means of the vertical seismometer.

"4th. To obtain by a combination of the above results the magnitude and direction of the oblique undulations.

"From the indications of these two apparatuses the observations of the terrible phenomena which plunged Manila into the greatest affliction were deduced and given each day. We do not place absolute reliance in them, because the apparatuses can only offer certainty in their indications in cases where the complications and violence of the present movements are absent; but we believe they have a relative and not inconsiderable value, and that they give a sufficiently exact idea of the occurrence, for which we consider them very useful in a comparative point of view, particularly to those who have experienced the terrible phenomena.

"After these preliminary remarks, we will give the different observations collected each day, which will prove more intelligible by consulting the various figures traced by the pendulum, and these in their turn will prove a complement to those observations.

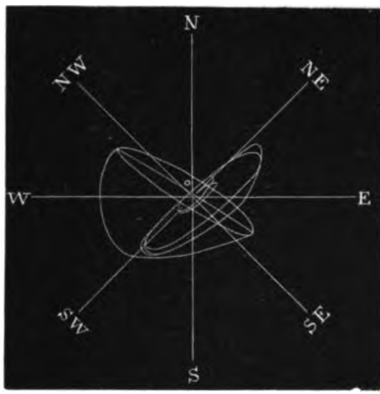
"In the months of April and May, commotions began to be felt in the northern provinces of Luzon; the centre of the seismic oscillation, according to different indications received at this station, seemed to coincide with a volcano extinct for some time, and situated between Lepanto and Abra, in the central range of Luzon, in lat. 16° 22′ N. and long. 127° E. of the Observatory of San Fernando.

"In the beginning these disturbances were weak and infrequent; but in the month of June their intensity sensibly increased, extending to the north and south in a much larger zone. The direction never changed; and if in some telegrams certain contradictions were observed, these appear to have been the effect of the haste with which the observations were taken, which can never be relied on where there are no special instruments used to mark a change in the direction of a seismic oscillation. In the beginning of July shocks were also felt; but

from the 5th to the 14th no notices of earthquakes in any part of the island were received in Manila.

"On the 14th, at 12 h. 33 m. P.M., being threatened with a storm from the north-east, indicated by an extraordinary fall of the barometer, we experienced here the first shock, in which we observed that two centres of oscillation were combined (see fig. No. 1), one on the second quadrant, from whence the pendulum of the horizontal seismo-

FIG. 1.



meter began to oscillate; and the other from the third, where the oscillation of this first movement terminated, and which was chiefly horizontal; the amplitude of total oscillation reached 5° 25′. The horizontal pendulum inscribed a cross with arms almost at right angles, the first bearing from S.E. 10° E. to N.W. 10° W.; the second from S.W. 5° W. to N.E. 5° E.

"The first impulse was in the direction from south-east to north-west. The amplitude of this oscillation described an arc of 5° 25′, and appeared to be formed by the first shock, because the pendulum almost at once oscillated in a direction almost perpendicular to the first. The amplitude of this second oscillation was somewhat less than that from the first impulse.

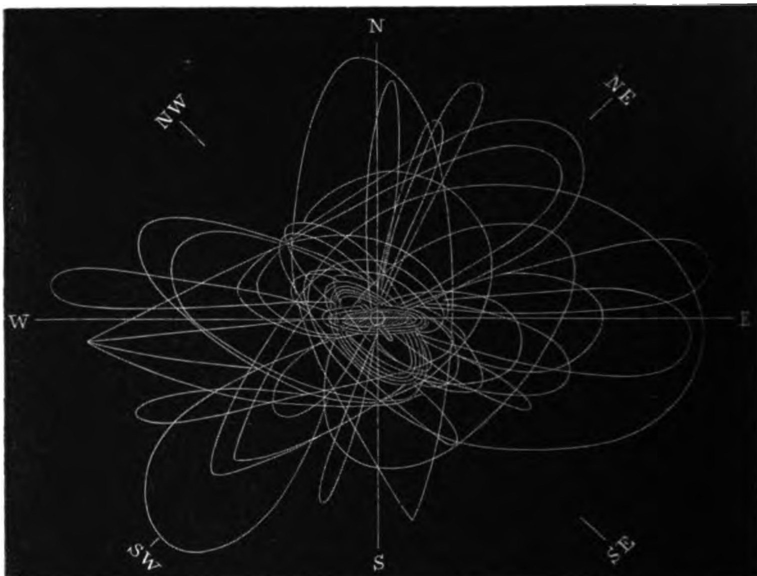
"The index of the vertical seismometer departed 4 millims. from its position. After this first movement, we experienced two more shocks in the space of an hour and a-half; on the 15th and 16th there were no perceptible movements; on the 17th, two slight shocks were felt.

"It was on the 18th, at 12 h. 40 m. P.M., when the great earthquake of oscillation, trepidation, and what is commonly called rotatory movement, occurred simultaneously. Its duration was 1 m. 10 s.

"It is impossible to describe all the movements of the pendulum on account of their number and variety. We shall therefore confine our-

selves to the principal directions with their amplitudes. All the movements are, however, to be seen in fig. No. 2.

Fig. 2.



"It is to be noted that, from our point of view, only the great oscillation from east to west (which was the most measured and free from violent shocks), indicates the true inclination of the building towards the west.

"1. The greatest oscillation from E. 5° S. to W. 5° N.; the amplitude of the greatest oscillation 22°, or by seismic wave motion 11° to east and 11° to west.

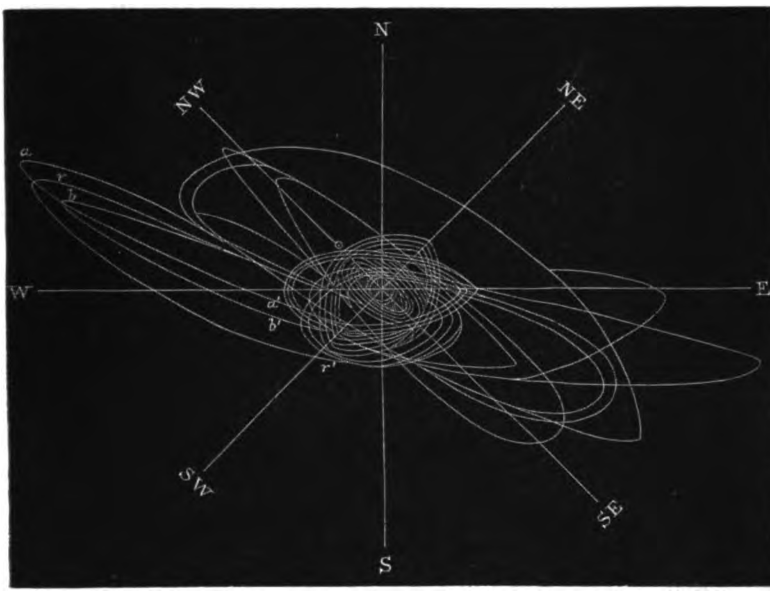
"2. Greatest oscillation from south-west to north-east, true, amplitude 19°, but with the difference of more inclination towards the south-west, which reached to 10° 10′, against 8° 50′ only towards the north-east.

"3rd. Greatest oscillation from N. 4° W. to S. 4° E., amplitude of this oscillation 16°, in which also is observed that the inclination is greater towards the south, being 9° south and only 7° north, the impulse consequently appears to be from north to south.

"The index of the vertical seismometer departed 34 millims. from its position. From the time of this earthquake until the 20th at three in the afternoon, at which time we experienced a very strong repetition, an uninterrupted series of small shocks were felt, which indicated that

we continued to be under the influence of the phenomenon. In this first repetition the oscillation and trepidation movements only were experienced, but with extraordinary violence. The oscillation of the pendulum was directed from S.E. 15° E. to N.W. 15° W. The amplitude described an arc of 12° 30′, but with the following peculiarity, that this did not consist of a perfect oscillation, but of three semi-oscillations, which show plainly the violence of the shocks (see in fig. 3 the lines marked with the letters *aa′*, *bb′*, *rr″*). The pendulum in

FIG. 3.

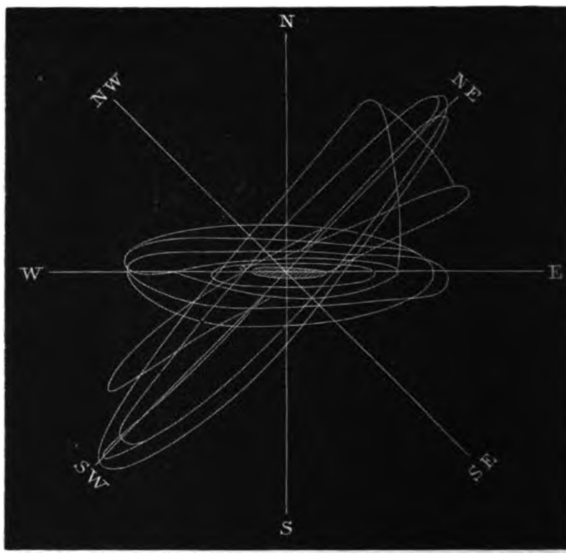


the first impulse from south-east to north-west reached the altitude indicated by the line *aa′*; on returning to its point of departure it received a new impulse which not only destroyed the velocity which it had acquired in its descent, but obliged it to ascend a second and third time almost to the same height it had attained by its first impulse.

"It is true that the inclination of the buildings was not equal to the deviation of the pendulum, but who is competent to understand the terrible convulsions which the former underwent during such repeated and violent shocks? The three above-mentioned convulsions, joined to the vertical undulation which reached 24 millims., being taken into account, the only thing to be astonished at is, that many more buildings were not thrown down. The pendulum continued oscillating during the whole of the evening from north-east to south-west.

"At 10 h. 40 m. P.M., the second, and very severe repetition took place, which, although of great violence, presented peculiarities distinct from the others; in those preceding, it was observed that the strongest focus of seismic radiation was in the second quadrant; in this latter it began, it is true, in the east, but with much less intensity than before; and the focus observed in the first quadrant continued with the same and even greater violence (see fig. 4). In this we perceive that the

Fig. 4.



oscillation from east to west, true, has an amplitude of 10° 5′ to the east, and 5° to the west; whereas, in the direction from north-east to south-west it comprises an arc of 17°: 9° to south-west, and 8° to north-east.

"The vertical seismometer marked 28 millims. Vibrations continued, but decreasing in frequency and above all in force.

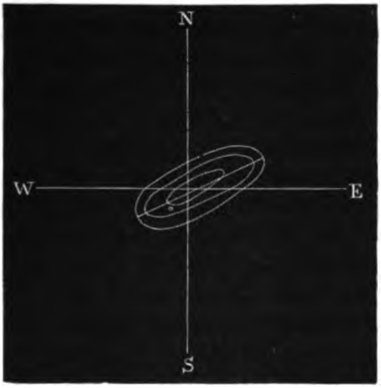
"The pendulum, which had never been at rest since the 18th until 3 P.M. of the 21st, in three succeeding days had long intervals of complete immobility. On the 25th, at 4 h. 2 m. A.M., another slight shock was felt; this, although of small intensity, we have faithfully transmitted to paper, because, in our opinion, it is important to show the gradual change of focus of seismic radiation during the whole time. The direction of the undulation was from E. 26° N. to W. 26° S., and only reached a total amplitude of 3° 45′. The movement of trepidation was inappreciable, as the index of the vertical pendulum departed only 0·7 millim. from its normal position.

"We will now recapitulate briefly what we understand by the figures.

"On the 14th, which is that represented in fig. 1, we notice two foci of seismic radiation, the first situated in the second quadrant, wher e it began, and the second situated in the first quadrant, where it terminated. In the earthquakes of the 18th we also discover the same two foci; but two others also appear which impelled the pendulum in every imaginable direction, as may be seen in fig. 2.

"Proceeding to that of 3 P.M. on the 20th, we find that the focus of the second quadrant acted with astounding violence, and the others disappeared (fig. 3). We turn now to fig. 4, which represents to us the violent repetition of 10 P.M. on the 20th, and we observe a great variation with regard to the foci of seismic radiation. In it we see that the oscillations from east to west, and which correspond to the focus, which before had acted with so much violence, were gradual and of much less intensity. On the contrary, that from north-east to south-west showed a great degree of undulation from these points.

FIG. 5.



"Finally, in fig. No. 5, which represents the last important oscillation on the morning of the 25th, we only note the focus of seismic radiation of the first quadrant operating with very little intensity, the other foci having entirely disappeared. We do not care at present to form deductions from the above observations, we have preferred to present them to general notice in order that scientific persons might form their own conclusions without being biassed by our opinions.

"Note 1st. Observe, that in speaking of the swing of seismic undulation from both sides of the centre of reference (place of instrument), we do not mean to say that the buildings moved from one side to the other like the pendulum, because it is very clear that the latter was only moved in *one* of the semi-undulations by the effect of the

impulse, or inclination, of the building, the *other* being the effect of the velocity acquired in the first semi-oscillation. The object of alluding to the double motion on either side of the centre of reference was to give freedom to the opinion which some people held, that earth-waves are similar to waves of sound in the air; while others aver that they are caused by the rising and sinking of the ground in localities more or less distant from the post of observation.

"Note 2nd. A great number of lines will be observed in the figures which appear not to be connected with the rest; we can only explain the fact as being caused by frequent vertical shocks, jerking the pendulum violently and causing it to leave one curve to follow another commenced by the new impulse.

"We can assure our readers that the curves as they are represented in the different figures were transferred from the lycopodium to the paper with the greatest fidelity."

This ends Father Faura's observations on the earthquakes, and in forwarding these he informs me that he will later on publish a more complete account. He is also engaged in establishing stations for meteorological observation in various parts of Luzon—which will be in telegraphic communication with Manila—and thence with Hong Kong; the chief object being to announce the advent of typhoons. As these storms invariably travel from about east by south, or east-south-east to west by north, or west-north-west, the Philippine islands, especially Luzon, are well situated for the object of giving storm warnings to the coast of China.

No doubt these stations will also be supplied with instruments, and receive instructions to observe the direction and force of earth-waves. If such stations had existed in July, the accounts would have been more complete and useful, and although reports from many places are recorded, they are in most cases unreliable and contradictory, and deal chiefly with the destruction caused to buildings, loss of life, and injury to persons.

The captain of the British steam-ship "Esmeralda," then at anchor in the Bay, states that in the earthquake at 6 P.M., on the 20th July, "the water bubbled and boiled up noisily all around the ship and the vessel tossed as if in a heavy gale;" that the wreck of a ship (which had been sunk for some years) "was thrown right up out of the water and one of her iron masts was seen to give way." He describes the sensation on board ship as well as on shore as that "of being suddenly connected with a galvanic battery strongly charged," and as being a "tremendous strain on the nerves."

Accounts are also given of fissures in the ground in various places, from which sand and water were emitted, especially in the neighbourhood of the Laguna de Bay, where hot sulphur springs have always existed.

I will now give a translation of a letter to a local paper from a resident near the spot, giving an account of the behaviour of the Volcano of Taal before and during the earthquakes of July. This letter was written in consequence of exaggerated reports of great eruptions of that volcano, and professes to give a true relation of the facts.

"The crater of the Volcano of Taal ceased to send up smoke as usual on Monday, 12th July. At nightfall on Wednesday the 14th, subterranean noises were heard, and a heavy swell was observed in the lake" (the volcano is on an island in the Lake of Taal), "which ceased after the earthquake of the same night; louder subterranean noises were heard during the earthquake.

"On Thursday the 15th, two columns of smoke continued to issue, with intervals, until the 16th, when they almost disappeared, and the volcano subsided to its usual state.

"On Sunday the 18th, in the neighbouring villages of Tanauan, Sto. Tomas, and Talisay, and nearly as far as Lipa, a fog of smoke, with smell of sulphur, was observed, which disappeared suddenly at noon; a short time after occurred the violent earthquake of Sunday, at 12 h. 40 m. P.M. In the afternoon of the same day, the 18th, the volcano again threw up the two columns of smoke, at intervals, until Tuesday the 20th, at 10 h. A.M., when it ceased smoking entirely. In the afternoon, at 3 h. and some minutes, the violent earthquake, felt at Manila, which was also most intense at Batangas and towards the Laguna, occurred. From eight to ten on Tuesday night, a brightness was seen over the volcano, as if reflecting the light of fire from the crater on the vapours which arose from it. This brightness ceased suddenly and the atmosphere cleared entirely, and at ten began the strong shocks of earthquakes, the first being the violent shock felt in Manila. These continued in the Province of Batangas" (in which Taal is situated) "during the night and were sensible for several days.

"On Wednesday the 21st, in the morning, the volcano threw up a great quantity of smoke, to a considerable height, in one column, the whole size of the crater, and continues to do so up to the time of writing this notice.

"In the evening, the Volcano of Maquiling, which had been considered extinct, gave forth much smoke, which caused terror in the province, because the people feared the crater there would break out again, and they called to mind the terrible eruption of the Volcano of Taal, on 12th December, 1754, when the lava destroyed the villages of Tananay, Sapa, Lipa, and Taal, which villages were afterwards rebuilt in a position more remote from the volcano."

Among other strange phenomena recorded, it is stated from other sources that the great mountains of Banajoa, Maquiling, and San

Cristobal were observed at the time of the earthquake to be covered by clouds of, to all appearance, gaseous vapour; and the Padre Bravo, Curate of Lilio, asserts, that the movement of Banajoa was so awful to behold that residents of that village, situated at the base of the mountain, feared that it would fall over and bury them beneath it.

The two sheets containing diagrams of the five principal shocks were lithographed at Manila, under the careful supervision of Father Faura, and I thought it better to send them as received rather than attempt a tracing, the lines being so complicated. I have not appended a translation of the few descriptive notes on the sheets, as the terms used are almost identical with their English signification.

February 24, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Simple Mode of Eliminating Errors of Adjustment in Delicate Observations of Compared Spectra." By Professor G. G. STOKES, Sec. R.S. Received February 12, 1881.

When the identity or difference of position of two lines, bright or dark, in the spectra of two lights from different sources has to be compared with the utmost degree of accuracy, they are admitted simultaneously into different but adjacent parts of the slit of a spectroscope and viewed together. It was thus, for instance, that Dr. Huggins proceeded in determining the radial component of the velocity of the heavenly bodies relatively to the earth. It is requisite that the two lights that are to be compared should fall in a perfectly similar manner on the slit: and it will be seen, from a perusal of his paper, how careful Dr. Huggins was in this respect.

In a paper read before the Royal Society on the 3rd instant, Mr. Stone has proposed to make the observation independent of a possible error in the exact coincidence of the lights compared by constructing a reversible spectroscope, by which the light should be refracted alternately right and left, supposing for facility of explanation the slit to be vertical.

The idea is an elegant one, but I apprehend that there would be considerable difficulty in carrying it out. For a spectroscope giving large dispersion is of considerable weight, and the reversal of so heavy an apparatus would be liable to introduce possible errors arising from flexure.* It would be difficult to make sure that such did not exist, at any rate, unless the instrument were constructed with great nicety and firmness, which would add considerably to the cost; and even then the care and time required for the reversal would help to obliterate the observer's memory of what he had seen in the first position of the instrument.

A method has occurred to me of effecting the reversal without reversing the spectroscope, but merely giving a lateral push to a little apparatus which need not weigh more than a few grains.

If the base of an isosceles prism be polished as well as the sides, and a ray of light parallel to the base and in a plane perpendicular to the edge fall on one of the equal sides of the prism so as to emerge from the other, after suffering an intermediate reflection (which will necessarily be total) at the base, its course after refraction will be parallel to its course before incidence; and there will, moreover, be no lateral displacement, provided the lateral distance of the base from the incident ray be such that the point of reflection is at the middle of the base.

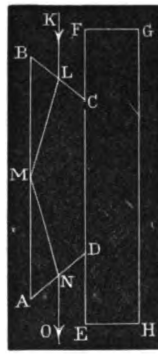
If the slit of the spectroscope be covered by such a prism, placed close to the slit and facing the collimating lens, to the axis of which its base is parallel, it will not disturb the general course of the light incident on the spectroscope, nor even produce a lateral displacement provided the lateral position be that mentioned above; but in consequence of the reflection there will be a reversal as regards right and left, and any error in the placing of the lights to be compared will thus be detected and eliminated, by comparing the spectra seen with the light from the slit direct or reflected. If the prism be placed quite close to the slit it may be made very minute in section, though it should be long enough to cover the slit, and then the change of focus which it produces will be insignificant.

There will be no need, however, to make the prism so very minute, nor to place it so close to the slit, provided it be associated with a plate to take its place in the direct observation, and compensate for the change of focus which is produced by its introduction.

Let ABCD be a section of the prism, let M be the middle point of the base AB, KLMNO the course of a ray passing as above described, which is supposed to be the axis of the pencil coming through the

* After the present paper was sent in to the Society, I was informed by Mr. Stone that the spectroscope he had in his mind was a direct-vision one, which could be turned in its socket, the slit and cylindrical lens remaining fixed. To such an instrument the objection as to flexure would not apply.

middle of the slit. Let ϕ be the angle of incidence, which will be half the angle of the prism, and the complement of either angle A or B, ϕ' the angle of refraction, μ the index of refraction, b the



base AB, l the length of path, LM+MN, of the ray within the glass, p=LN. In spectroscopic work it is the focus of rays in the primary plane that we have to deal with; and we get for the shortening (s) of the focus, or, in other words, the distance by which the slit is virtually brought nearer to the collimating lens,

$$s=p-l\frac{\cos^2\phi}{\mu\cos^2\phi'}.$$

But since MBL$=90°-\phi$ and MLC $90°-\phi'$ we have

$$l=b\frac{\cos\phi}{\cos\phi'};\qquad \text{also } p=l\cos(\phi-\phi');$$

whence $$s=b\left\{\cos(\phi-\phi')\frac{\cos\phi}{\cos\phi'}-\frac{\cos^3\phi}{\mu\cos^3\phi'}\right\}=t\left(1-\frac{1}{\mu}\right),$$

where t is the thickness of a compensating plate which shall produce the same shortening of focus. In the figure, the part of the prism which is out of use is represented as cut away, to make the instrument more compact, and EFGH represents the compensating plate. The faces CD of the truncated prism, and EF, HG, of the plate, of course need not be polished, and had better perhaps be blackened.

In the figure I have taken 80° for the angle of the prism, and supposed μ to be 1·52, which data give t=1·225b, nearly. A blunter angle would have made the instrument a little more compact in the direction AB, but I wished to avoid needless loss of light by the two reflections that accompany the refractions. The size of the prism and compensating plate must depend upon its distance from the slit, and

the angle subtended at the slit by the objective of the collimator. It should be a little larger than what is just sufficient to take in the largest pencil that is to be observed, but not beyond that. The object in keeping it as small as conveniently may be, is that only a trifling change of focus may be required when the instrument is pushed aside altogether, and the slit viewed directly through the spectroscope, without the slight loss of light due to the two reflections.

The compensating plate is represented as placed at the narrow end of the prism, which permits of the two being cemented together, thereby facilitating the support. I do not think that the minute quantity of light which is reflected at L, and scattered at the surface (even though blackened) FC in such a direction as to mingle with the direct light would be any inconvenience, being too faint to be visible at all. If it were wished to avoid this, or to get more easy access to the surfaces AD, BC, for cleaning if requisite, the plate might be placed at the other side; but in that case it must not be cemented to AB, as that surface is wanted for total reflection.

The little instrument I have suggested may conveniently be called a *slit-reverser*, to distinguish it from other arrangements which have been proposed, and in which the spectrum itself is reversed.

P.S. Feb. 21.—The method proposed above is more directly applicable to such an object as the comparison of really or apparently coincident lines in the spectra of two elements than to astronomical measurements, because in the latter case a great part of the difficulty arises from a want of perfect accuracy in the clockwork movement of the equatoreal. Yet I cannot help thinking that even for astronomical work the method will be found useful; for we can pass in a moment from the direct to the reflected image of the slit, and *vice versâ*, and by taking the measures alternately in the two modes, and combining them exactly as in weighing with a balance that is still swinging, any error progressive with the time would tend to be eliminated.

II. "Notes on Physical Geology. No. VII. On the Secular Inequalities in Terrestrial Climates depending on the Perihelion Longitude and Eccentricity of the Earth's Orbit." By the Rev. SAMUEL HAUGHTON, Professor of Geology in the University of Dublin. Received February 19, 1881.

The attention of geologists was first called by M. Adhémar, and afterwards more fully by Mr. James Croll, to the possible importance of these long inequalities in climate, in explaining the climates of geological periods, which differ considerably from those of the present

time in the same places; but, so far as I know, no one has written down these inequalities in a mathematical form, or calculated numerically the effects upon climate they are capable of producing. I shall attempt to do so in the present note.

The temperature of any place at any time depends upon sun-heat and terrestrial radiation, and involves the solution of the following differential equation:—

$$\frac{d\theta}{dt}=\frac{A\phi(z)}{r^2}-B(\theta+a) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where θ=temperature of place in Fahrenheit degrees,

A=a solar constant,

$\phi(z)$=a known function of the sun's zenith distance,

r=distance of earth from sun,

B=a local atmospheric constant depending on latitude and local conditions,

a=another local atmospheric constant depending on latitude and local conditions.

When the absorption of sun-heat by the atmosphere is neglected

$$\phi(z)=\cos z,$$

and the first term of the equation can be integrated for one day in terms of t, and afterwards summed for all latitudes, for one year; but when we take account of the absorption of sun-heat by the atmosphere, $\phi(z)$ becomes an exponential function of $\cos z$, that cannot be integrated, even by series.

It has been shown, however, that, whether we take account of the atmospheric absorption or not, the quantity of sun-heat represented by $\Sigma\frac{A\phi(z)}{r^2}$ taken for the whole summer is the same, at similar latitudes, in the northern and southern hemispheres; and, in like manner, the winter sun-heats are the same in the two hemispheres, whatever may be the perihelion longitude and eccentricity of the earth's orbit.

This being so, the secular inequalities of climate under discussion, must arise from the second term of equation (1), depending on terrestrial radiation, and ultimately on the fact that the summers and winters in the two hemispheres differ in length, that difference depending on the perihelion longitude and eccentricity, as is well known.

Before discussing the second (or radiation) term of equation (1), I shall explain my reasons for assuming it to have the form

$$-B(\theta+a),$$

and show what that form implies.

The heat received from the sun by the layer of atmosphere next the ground is dissipated in the following ways:—

1. By convection, or ascent of the warmer surface air into the higher layers of the atmosphere, until it is cooled down to a temperature* a, such that its direct radiation into space is equal to the heat received from the sun and lower layers of the atmosphere at that height in the atmosphere.

2. By conduction, or transfer of heat from layer to layer, as in solids.

3. By direct radiation from the surface layer across the atmosphere into star-space.

The third of these sources of loss of heat may be neglected, because the atmosphere, which absorbs about one-fourth of the luminous sun-heat passing through it vertically, will absorb nearly all non-luminous heat and allow only an infinitesimal fraction to escape into star-space.

The first source of loss of heat (convection) is much more influential than the second (conduction), but for my present purpose it is not necessary to make any distinction between them, for the loss of heat from both causes will follow the same law and is proportional to the difference of temperature between the surface layer of the atmosphere and the upper, or equilibrium control layer; that is, the loss of heat is proportional to $(\theta+a)$.

The radiation is therefore represented by

$$\text{Radiation}=\text{B}(\theta+a)dt.$$

This expression cannot be summed without assuming some relation between θ and t, which I do as follows:—It is well known that the diurnal temperature does not reach its maximum and minimum at midday and midnight, but some hours after the sun's passage of the upper and lower meridian; and it is also well known that the hottest and coldest days of the year do not occur at midsummer and mid-winter, but some days after.

This law of diurnal and annual change of temperature presents so close an analogy to the law of the diurnal and annual tides, as to justify us in assuming for its mathematical expression formulæ similar to those of the well-known equations of the diurnal and annual tides.

I therefore assume—

$$\theta=\theta_0+\begin{matrix}a_1 \cos h\\ b_1 \sin h\end{matrix} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where θ_0= mean diurnal temperature,

h= sun's hour angle.

* As a is probably below zero for all latitudes, I shall reckon it positive below zero, and θ positive above zero.

Integrating (2) for one day, we have—

$$\text{Diurnal radiation} = 2\pi B(\theta_0 + a) \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Again, assuming

$$\theta_0 = \Theta_0 + {}^{\alpha_1}_{\beta_1}{}^{\cos l}_{\sin l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

where Θ_0 = mean annual temperature,

l = sun's longitude.

Summing for the whole year, we find—

$$\text{Annual radiation} = \Sigma\left((\Theta_0 + a) + {}^{\alpha_1}_{\beta_1}{}^{\cos l}_{\sin l}\right)\delta t \quad . \quad . \quad . \quad . \quad (5),$$

but, by the theory of the earth's motion, we have—

$$\delta t = \frac{T(1-e^2)^{\frac{3}{2}}}{2\pi} \times \frac{\delta\theta}{(1 + e\cos\theta)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

$$l = \theta + \varpi,$$

where T = earth's periodic time of revolution,

e = eccentricity of earth's orbit,

θ = true anomaly,

ϖ = earth's perihelion longitude.

Substituting in (5) and summing, we find, for the year,

$$\text{Annual radiation} = 2\pi BT(1-e^2)^{\frac{3}{2}} \times \left\{\left(\Theta_0\left(1 + \frac{3e^2}{2}\right) + a\right) \pm \left({}^{\alpha_1}_{\beta_1}{}^{\cos \varpi}_{\sin \varpi}\right)e\right\} \quad . \quad . \quad . \quad . \quad (7),$$

or, neglecting quantities of the order of the square of the eccentricity,

$$\text{Annual radiation} = 2\pi BT \times \left((\Theta_0 + a) \pm \left({}^{\alpha_1}_{\beta_1}{}^{\cos \varpi}_{\sin \varpi}\right)e\right) \quad . \quad (8).$$

From this equation, we find—

$$\text{Secular range of temperature} = 2e\sqrt{\alpha_1^2 + \beta_1^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

$$= e \times \text{Annual range of temperature.}$$

From this we may calculate the secular range of temperature in the northern and southern hemispheres, with the following results:—

Northern Hemisphere.

Lat. N.	Annual Range.	Present Secular Range.	Maximum* Secular Range.
0°	2°·2 F.	0°·037 F.	0°·185 F.
10	4·5 „	0·075 „	0·375 „
20	13·2 „	0·220 „	1·100 „
30	24·8 „	0·413 „	2·065 „
40	33·0 „	0·550 „	2·750 „
50	44·2 „	0·737 „	3·685 „
60	55·3 „	0·922 „	4·610 „
70	59·8 „	0·997 „	4·985 „
80	59·1 „	0·985 „	4·925 „

The secular range of mean annual temperature in the southern hemisphere is as follows:—

Southern Hemisphere.

Lat. S.	Annual Range.	Present Secular Range.	Maximum Secular Range.
0°	2°·2 F.	0°·037 F.	0°·185 F.
10	7·0 „	0·117 „	0·585 „
20	10·5 „	0·175 „	0·875 „
30	13·3 „	0·222 „	1·110 „
40	11·8 „	0·197 „	0·985 „
50	8·5 „	0·142 „	0·710 „
60	6·5 „	0·108 „	0·540 „

As extreme examples of the respective types of climate in the northern and southern hemispheres, I may give the result of my calculations for North Grinnell Land and Kerguelen Island.

	Lat.	Present Mean Annual Temperature.	Maximum Secular Range.
North Grinnell Land	81° 44′ N.	−2°·42 F.	6°·5 F.
Kerguelen Island....	50 0 S.	+40·7 „	0·9 F.

* Because the maximum eccentricity of the earth's orbit is $\frac{1}{13}$th instead of $\frac{1}{60}$th, as at present.

III. "Further Experiments on the Action of an Intermittent Beam of Radiant Heat on Gaseous Matter. Thermometric Measurements." By J. TYNDALL, F.R.S. Received February 21, 1881.

In the concluding paragraph of the note communicated on the 10th of January to the Royal Society these words occur:—"The vapours of all compound liquids will, I doubt not, be found sonorous in the intermittent beam." Since that time I have examined eighty different liquids, both at the ordinary temperature of the air and at their boiling temperatures, and have verified so far the prediction just quoted. In all cases I have obtained musical sounds—some feeble, some moderate, and some exceedingly strong. I have, moreover, determined by thermometric expansion the absorptions exerted by the vapours of more than twenty of these liquids, and it is my intention to subject the whole of them to this test. The harmony and mutual support exhibited by two series of experiments, conducted in accordance with these two diverse methods, are on the whole admirable. The investigation, however, is laborious, and many weeks must elapse before I am able to submit it *in extenso* to the Royal Society.

Tested by the thermometric method, my old experiments on aqueous vapour again affirm their validity. A long and narrow glass tube, bent into the form of a U, was partially filled with coloured water. One leg of the U was connected with the recipient which contained the gaseous substances submitted to experiment, while the other end was left open to the air. Before permitting the beam to pass, the liquid stood at the same level in both legs of the tube. Cleansing the recipient thoroughly, and filling it with well-dried air, a powerful beam was sent through it. There was no sensible expansion and consequently no perceptible motion of the thermometric column.

Air similarly dried was then passed over bibulous paper, moistened by water of a temperature of 14° C. The modicum of vapour carried forward at this temperature by the dried air, when smitten by the beam, produced instantly a depression of 55 millims. in the leg of the tube connected with the recipient, and an equal elevation in the other leg. The difference of level in the two legs amounted, therefore, to 110 millims. No trace of haze or sign of condensation could be detected within the recipient. Its boundaries were as bright, and its contents as free from turbidity, as when the dry air alone was employed.

With a conical tube of a certain size, stopped at its base by a transparent plate of rocksalt, I have obtained a considerable intensification of the sounds. Abandoning the ear-tube altogether, and filling the

hollow cone with olefiant gas, its music has been heard at a distance of 18 feet from the source of sound.

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Wace (Rev. Henry) On the Calculation of Logarithms. 8vo. [*London* 1873.]

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THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read, as follows:—

Alderson, Henry James, Col. R.A.
Ayrton, Prof. William Edward.
Bates, Henry Walter.
Bristowe, John Syer, M.D., F.R.C.P.
Browne, James Crichton, M.D., LL.D., F.R.S.E.
Buchanan, George, M.D., F.R.C.P.
Christie, William Henry Mahoney, M.A., Sec. R.A.S.
Clarke, Charles Baron, M.A., F.L.S., F.G.S.
Creak, Ettrick William, Staff-Commander, R.N.
Dallas, William Sweetland, F.L.S.
Darwin, Francis, M.A., M.B., F.L.S.
Day, Francis.
Day, John, M.D.
Dickie, Prof. George, A.M., M.D., F.L.S.
Dobson, George Edward, Surgeon-Major, M.A., M.B., F.L.S.
Douglass, James Nicholas, M.I.C.E.
Foster, Prof. Balthazar Walter, F.R.C.P.
Glazebrook, Richard Tetley, M.A.
Goodeve, Prof. Thomas Minchin, M.A.
Grantham, Richard Boxall, F.G.S., M.I.C.E.
Groves, Charles Edward, F.C.S.
Herschel, Prof. Alexander Stewart.
Hutchinson, Prof. Jonathan, F.R.C.S.
Kempe, Alfred Bray, B.A.
Lee, John Edward, F.S.A., F.G.S.
Ley, Rev. W. Clement, M.A.
Liversidge, Prof. Archibald, F.G.S., F.C.S., F.L.S.
Macalister, Prof. Alexander, M.D., Sec. R.I.A.
McLeod, Prof. Herbert, F.I.C., F.C.S.
Miller, Francis Bowyer, F.C.S.
Niven, William Davidson, M.A.
Ord, William Miller, M.D., F.L.S., F.R.C.P.
Palgrave, Robert Henry Inglis, F.S.S.
Phillips, John Arthur.
Preece, William Henry, C.E.

Pritchard, Urban, M.D., F.R.C.S.
Radcliffe, Charles Bland, M.D., F.R.C.P.
Ransome, Arthur, M.A., M.D.
Ranyard, Arthur Cowper, F.R.A.S.
Rawlinson, Robert, C.B., M.I.C.E.
Reinold, Prof. Arnold William, M.A.
Rodwell, George F., F.R.A.S., F.C.S.
Samuelson, Bernhard, M.I.C.E.
Spiller, John, F.C.S.
Stoney, Bindon Blood, M.A. M.I.C.E.
Tidy, Prof. Charles Meymott, M.B., M.R.C.S., L.S.A.
Traquair, Ramsay H., M.D.
Warren, Charles, Lieut.-Col. R.E.
Watson, Rev. Henry William, M.A.
Williams, Charles Theodore, M.A., M.D., F.R.C.P.
Wright, Charles R. Alder, D.Sc.
Wright, Edward Perceval, M.A., M.D., F.L.S.

The following Papers were read:—

I. "Some Experiments on Metallic Reflexion. No. II." By Sir JOHN CONROY, Bart., M.A. Communicated by Professor STOKES, Sec. R.S. Received February 12, 1881.

Professor Stokes did me the honour of communicating to the Royal Society a short paper ("Proc. Roy. Soc.," vol. 28, p. 242) giving an account of some determinations I had made of the values of the principal incidence for, and the principal azimuth of, red light, when reflected by a gold plate in contact with air, water, and carbon bisulphide.

These experiments I have continued, using light of different refrangibilities, and gold and silver plates, and also thin films of these metals.

The observations were made by causing a beam of light which had been polarised elliptically, by passing through a nicol with its principal section at an angle of 45° with the plane of incidence, and a "quarter undulation plate," with one of the neutral axes in that plane, to fall on the metal plate, and then altering the angle of incidence till the reflected light was plane polarised.

The angle of incidence at which this was the case was different, according as one or other of the neutral axes of the quarter plate was in the plane of incidence, and the mean of these two values was taken as the true principal incidence.

The goniometer used in these experiments was described in the former paper, but had an additional vertical divided circle attached to the inner end of the tube fixed to the collimator arm, in order to facilitate the adjustment of the quarter plate, and to permit the plate being changed, without the necessity of determining each time the position of the neutral axes.

Brewster's law, that the tangent of the angle of polarisation is equal

to the refractive index of the medium, not having been, as far as I was aware, ever experimentally verified in the case of transparent bodies in contact with media other than air, it appeared desirable to make some experiments on this point, though there was every reason for supposing that the law would be found to hold good.

A crown glass prism was fastened to the vertical stage, the quarter undulation plate and the analysing nicol removed, the polarising nicol placed with its principal section in the plane of incidence, and the angle of polarisation determined, a paraffine lamp being used as the source of light. The thin glass vessel was then placed on the vertical stage, and filled successively with water and carbon tetrachloride, and the angles of polarisation again determined.

Glass Prism.

	° ′	° ′
Angle of polarisation in air	57 14	57 02
" " water	49 41	49 43
" " carbon tetrachloride	46 32	46 23

The angles of polarisation in air are the mean of twelve and four observations respectively, those in water of eight and twelve, and those in carbon tetrachloride of twelve observations.

When the surface of the glass was in contact with water or carbon tetrachloride, its reflective power was so much diminished that it was not possible to determine the angle at which the minimum amount of light was reflected, and therefore the angles at which the light ceased to be visible were observed, and the mean of these taken as the angle of polarisation.

The tangents of the angles of polarisation in water and carbon tetrachloride, when multiplied by the refractive indices of these liquids, give the following values for the angle of polarisation in air:—

	° ′	° ′
Angle of polarisation in air observed	57 14	57 02
Angle of polarisation in air calculated from observations in water	57 28	57 33
Angle of polarisation in air calculated from observations in carbon tetrachloride	57 01	56 52

The distilled water which was used in these experiments had not been freed from air, and this may account for the values of the angles of polarisation in air, calculated from the measurements made in water, being somewhat too high.

These observations show that, within the limits of experimental error, Brewster's law holds good for glass in contact with water and carbon tetrachloride, as well as air, and that in all probability it is universally true for transparent bodies.

The same gold plate which had been used in the previously described experiments was fastened to the stage of the goniometer, and the principal incidences and azimuths observed with yellow and blue light, when the plate was in air, water, and carbon bisulphide.

An alcohol flame with a salted wick and a paraffine lamp, with a glass trough filled with ammonio-sulphate of copper placed in front of it, were used as the sources of light. The light transmitted by the deep red glass used in the previous experiments and by the ammonio-sulphate was not homogeneous.

The light of the greatest intensity in the one case was slightly more refrangible than C, and in the other of a refrangibility nearly midway between F and G.

The polarising nicol was placed with its principal section at an angle of 45° with the plane of incidence in each of the four quadrants successively, and two observations made with the nicol in each position; so that the value of the principal incidence and the principal azimuth for each position of the "quarter undulation plate" is the mean of eight observations.

The following table contains the results of these experiments; the values for red light, which had been previously obtained, are included in it for the sake of comparison:—

		Quarter plate. I. P.I.	Quarter plate. I. P.A.	Quarter plate. VI. P.I.	Quarter plate. VI. P.A.	Corrected numbers. P.I.	Corrected numbers. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold in air, red light.	Plate at A.	80 13	35 52	78 03	35 43		
	Plate at B.	70 45	35 57	73 45	35 21		
	Mean value	75 29	35 54	75 54	35 32	76 0	35 27
Gold in air, yellow light.	Plate at A.	76 33	38 12	73 59	36 27		
	Plate at B.	68 33	37 39	72 55	36 20		
	Mean value	72 33	37 55	73 27	36 23	73 28	36 22
Gold in air, blue light.	Plate at A.	65 32	30 19	60 27	32 31		
	Plate at B.	69 03	29 44	71 51	32 09		
	Mean value	67 17	30 01	66 09	32 20	67 24	29 47
Gold in water, red light.	Plate at A.	76 46	37 13	74 46	36 0		
	Plate at B.	66 46	35 50	70 25	36 49		
	Mean value	71 46	36 31	72 35	36 24	72 46	36 23
Gold in water, yellow light.	Plate at A.	73 47	38 05	70 05	37 28		
	Plate at B.	65 29	38 27	68 51	36 22		
	Mean value	69 38	38 16	69 28	36 55	69 28	36 52
Gold in water, blue light.	Plate at A.	61 51	28 30	57 09	31 35		
	Plate at B.	65 26	29 06	70 59	31 01		
	Mean value	63 38	28 48	64 04	31 18	63 36	28 38

		Quarter plate. I. P.I.	Quarter plate. I. P.A.	Quarter plate. VI. P.I.	Quarter plate. VI. P.A.	Corrected numbers. P.I.	Corrected numbers. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold in carbon bisulphide, red light.	Plate at A.	76 10	37 48	71 37	36 59		
	Plate at B.	62 44	37 40	68 26	36 43		
	Mean value	69 27	37 44	70 01	36 51	70 03	36 48
Gold in carbon bisulphide, yellow light.	Plate at A.	71 0	39 09	67 02	37 17		
	Plate at B.	61 50	38 24	66 02	37 10		
	Mean value	66 25	38 46	66 32	37 13	66 32	37 12
Gold in carbon bisulphide, blue light.	Plate at A.	59 51	31 30	54 24	29 17		
	Plate at B.	61 30	33 13	67 56	31 08		
	Mean value	60 40	32 21	61 10	30 12	60 40	32 23

The last column gives the values as corrected by Professor Stokes' method (*loc. cit.*, p. 248).

The table shows, that for red and yellow light the observations made with plate I, and for blue light those made with plate VI, are but of little value; whilst for red and yellow light the observations made with plate VI, and for blue light those made with plate I, lie very close to the corrected numbers.

All subsequent experiments were therefore made with plate VI for red and yellow light, and with plate I for blue light.

The gold plate used in these experiments had been polished with rouge, and from time to time rubbed with a little rouge, and then with a clean chamois leather to remove the polishing powder.

From some experiments with a silver plate, it appeared probable that the optical constants of the plate might depend to a certain extent on the nature of the polishing powder used, and therefore the gold plate was dismounted and polished with putty powder, and then well rubbed with a clean chamois leather, and the principal incidences and azimuths determined.

Four observations were determined in each position of the quarter plate, two with the principal section of the polarising nicol on the right, and two on the left of the plane of incidence; the plate VI being used with red and yellow light, and plate I with blue light.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold plate in air.	Plate at A.	75 57	41 17	72 18	41 40	67 38	34 44
	Plate at B.	71 45	41 29	71 21	40 57	66 34	34 55
	Mean value	73 51	41 23	71 49	41 18	67 06	34 49

After an interval of two months the measurements were repeated, the plate being first rubbed with a clean chamois leather.

		Red light.		Yellow light.		Blue light.	
		P.I.	P.A.	P.I.	P.A.	P.I.	P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold plate in air.	Plate at A.	76 20	42 40	72 16	41 07	67 59	37 12
	Plate at B.	71 49	42 05	70 59	41 14	66 29	35 50
	Mean value	74 04	42 22	71 38	41 10	67 14	36 31

The measurements were then made with the plate in water and carbon bisulphide.

		Red light.		Yellow light.		Blue light.	
		P.I.	P.A.	P.I.	P.A.	P.I.	P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold plate in water. I.	Plate at A.	72 43	42 36	69 23	42 10	63 50	36 44
	Plate at B.	68 07	42 39	67 28	41 01	62 39	35 12
	Mean value	70 25	42 37	68 25	41 35	63 14	35 58
Gold plate in water. II.	Plate at A.	73 17	42 10	67 11	39 25	64 27	36 07
	Plate at B.	68 18	42 24	66 36	42 25	62 25	36 41
	Mean value	70 24	42 17	66 53	40 55	63 26	36 24
Gold plate in carbon bisulphide.	Plate at A.	71 52	42 37	66 58	41 26	61 41	36 19
	Plate at B.	66 57	42 29	66 14	41 56	58 29	37 35
	Mean value	69 24	42 33	66 36	41 41	60 05	36 57

The mean values of these two series of determinations are—

	Red light.		Yellow light.		Blue light.	
	P.I.	P.A.	P.I.	P.A.	P.I.	P.A.
	° ′	° ′	° ′	° ′	° ′	° ′
Gold in air	73 57	41 52	71 43	41 14	67 10	35 40
„ water	70 24	42 27	67 39	41 15	63 20	36 11
„ carbon bisulphide..	69 24	42 33	66 36	41 41	60 05	36 57

The values of the principal azimuths are considerably higher (about 6°) than those previously observed; and in the case of red and yellow light, when the gold plate was in air or water the principal incidences are lower (about 2°) than with the plate polished with rouge; but with blue light, and also when the plate was in contact with carbon bisulphide, there was but little difference in the values of the principal incidences.

The plate was again cleaned with rouge, and the measurements in air repeated.

		Red light.		Yellow light.		Blue light.	
		P.I.	P.A.	P.I.	P.A.	P.I.	P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Gold in air.	Plate at A.	76 41	41 07				
	Plate at B.	72 05	41 16				
	Mean value	74 26	41 11				
Gold in air.	Plate at A.	76 31	40 47	72 46	40 39	69 07	34 44
	Plate at B.	72 11	40 15	72 06	39 40	67 55	33 05
	Mean value	74 21	40 31	72 26	40 09	68 31	33 54

Polishing the plate with rouge appears to have had the effect of increasing the values of the principal incidences and diminishing those of the azimuths, but not to such an extent as might have been anticipated from the difference between the values obtained with the plate before and after it had been polished with putty powder. This may be due to the original smoothing and polishing with rouge having altered the surface of the plate to a greater extent than could be effected by repolishing with rouge the already polished plate.

The gold plate, which was a long narrow one, having been constantly rubbed in one direction whilst being polished, it was thought possible that the direction of the furrows produced by the polishing might influence the result: the plate was therefore turned so that these were perpendicular to the plane of incidence, instead of parallel with it. This alteration in the position of the plate did not appear to alter the value of the constants.

		Red light. P.I.	Red light. P.A.
		° ′	° ′
Gold in air..	Plate at A......	76 42	40 44
	Plate at B......	72 23	39 54
	Mean value.....	74 32	40 19

In order to obtain a gold surface free from the influence of any polishing powder, a sheet of gold leaf was placed between two pieces of paper, and the paper and gold cut into strips with a sharp pair of scissors, the gold leaf transferred to glass slips, floated out with water, in the manner described by Faraday, and the water allowed to drain off, leaving the gold leaf stretched out on the glass.

Some of the pieces of gold leaf were thinned by being floated on a dilute solution of potassium cyanide, the cyanide solution washed away with water as soon as the thickness of the gold had been sufficiently reduced, and the gold leaf left adhering to the glass.

Gold leaf treated in this way has a very fairly smooth surface, sufficiently good to act as a tolerable mirror, and both the ordinary leaf, and that thinned by the action of potassium cyanide, is transparent, the transmitted light being, as is well known, green.

In order to determine the thickness of the gold, the glass slips were carefully cleaned and weighed before the gold was transferred to them, and then, after being dried, again weighed.

The area of the gold was measured, and the specific gravity being taken as 19·36 ("Watts' Dictionary," vol. ii, p. 926), the thickness calculated.

The average thickness of gold leaf is stated in "Watts' Dictionary" (*loc. cit.*) not to exceed $\frac{1}{200000}$ of an inch (·0001270 millim.), and in "Roscoe and Schorlemmer's Chemistry" to be about ·0001 millim.,

numbers that agree very fairly well with those determined in the above-mentioned manner, especially when the actual weight of the gold (in no case exceeding ·0029 grm.) and the uncertainty as to its true specific gravity in the state of gold leaf is taken into account.

The glass slips were fastened to the goniometer, and the constants determined for the gold-air surface; four observations being made in each position of the quarter plate, two with the principal section of the polarising nicol on the right, and two on the left, of the plane of incidence; the plate VI being used with red and yellow light, and plate I with blue light.

No. of film.		Thickness.
2	....	0·0000645 millim.
7	....	0·0000709 ,,
5	....	0·0000837 ,,
4	....	0·0001107 ,,
3	....	0·0001135 ,,

		Red light.		Yellow light.		Blue light.	
		P.I.	P.A.	P.I.	P.A.	P.I.	P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
No. 2.	Plate at A.	74 43	40 50	71 05	40 51	68 18	37 21
	Plate at B.	70 39	41 14	69 26	40 11	65 54	37 02
	Mean value	72 41	41 02	70 15	40 31	67 06	37 11
No. 7.	Plate at A.	75 16	40 42	72 32	40 10	68 56	35 11
	Plate at B.	71 48	40 26	70 45	40 21	66 20	35 36
	Mean value	73 32	40 34	71 38	40 15	67 38	35 23
No. 5.	Plate at A.	76 44	43 27	73 59	41 45	69 51	38 30
	Plate at B.	72 54	43 30	72 41	42 22	67 51	38 16
	Mean value	74 51	43 28	73 20	42 03	68 51	38 23
No. 4.	Plate at A.	76 27	43 32	73 06	42 39	69 54	38 59
	Plate at B.	72 39	42 42	71 23	42 21	67 21	38 52
	Mean value	74 33	43 07	72 14	42 30	68 37	38 55
No. 3.	Plate at A.	76 34	42 56	73 20	42 02	69 49	39 09
	Plate at B.	72 22	42 55	72 16	41 59	67 16	38 31
	Mean value	74 28	42 55	72 48	42 0	68 32	38 50

Numbers 2 and 7 had been thinned with potassium cyanide, the other three (3, 4, and 5, being all cut from different leaves) were of their original thickness.

The table shows that, as the thickness of the leaf increases, the principal incidence and the principal azimuth increase, and to about the same amount for all three kinds of light, although only portions of the same leaf appear to be strictly comparable with each other.

The values obtained with the gold leaf and with the gold plate cleaned with putty powder agree fairly well together; the principal

incidences and azimuths being somewhat higher, especially with blue light, in the case of the gold leaf.

It therefore appears probable that of the two sets of values for the gold plate, those obtained after the plate had been polished with putty powder are nearest the truth.

A large number of determinations of the principal incidences and principal azimuths were made with a silver plate in air, water, and carbon tetrachloride, with both the quarter undulation plates.

The silver plate had been polished with rouge, and during the course of these experiments, which were carried on at intervals during some months, rubbed with a little rouge from time to time, and although afterwards well rubbed on a clean chamois leather, in certain lights it appeared to have a reddish tinge, although the surface was most brilliant.

In some few of the earlier experiments sixteen observations were made in each position of the quarter plate, four with the polariser in each quadrant; but most of the numbers are the means of eight observations, two for each position of the polariser.

The following table contains the measurements made with the quarter plate VI for red and yellow light, and with quarter plate I for blue light:—

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Silver in air. I.	Plate at A.	78 13	32 50	76 06	35 40	70 06	38 43
	Plate at B.	75 04	31 58	75 40	35 44	73 16	39 26
	Mean value	76 38	32 25	75 53	35 42	71 41	39 04
Silver in air. II.	Plate at A.	77 51	33 01	75 42	35 10	72 16	38 45
	Plate at B	74 0	32 47	75 21	34 59	72 24	38 46
	Mean value	75 55	32 54	75 31	35 04	72 20	38 45
Silver in water.	Plate at A.	75 13	33 42	73 11	36 47	67 09	40 06
	Plate at B.	71 16	33 12	72 20	35 27	69 08	40 07
	Mean value	73 14	33 27	72 45	36 07	68 08	40 06
Silver in carbon tetrachloride.	Plate at A.	75 04	32 52	70 27	35 41	64 32	39 36
	Plate at B.	71 01	32 34	70 50	35 22	67 32	39 10
	Mean value	73 02	32 43	70 38	35 31	66 02	39 23

The values of the principal incidences and principal azimuths were also determined by a somewhat different method. The principal section of the polarising nicol was placed successively in the plane of incidence, and at right angles to it, and the neutral axes of the quarter undulation plate (plate VI being used) at an angle of 45° with this plane.

The angle of incidence, and the azimuth of the analysing nicol by

which the light was reduced to a minimum, were observed; two such observations were made, and then the quarter plate turned through an angle of 90° and two more observations made, so that the values for each position of the polarising nicol are the means of four observations.

The retardation produced by the plate not being equal to 90° for the wave-length of the light employed, the resultant beam was elliptically polarised, the major and minor axes of the ellipse being in the plane of incidence, and in the plane perpendicular to it, or *vice versa*. The relative position of the axes depending on the position of the polarising nicol, and on the amount of retardation produced by the plate (*i.e.*, whether it was less or more than 90°).

An elliptical vibration being equivalent to two rectilinear vibrations in the planes of the axes, differing in phase by a quarter of an undulation, and of different amplitudes, this method affords a ready means of producing with the same retarding plate a difference of phase of exactly 90° between the vibrations in the plane of incidence and the plane perpendicular to it with light of different wave-lengths.

Hence the angle at which light polarised in this manner is reflected by a metallic plate as plane polarised light is the true angle of principal incidence; the azimuth, however, of the reflected light is not the principal azimuth.

The azimuths are different, both in sign and in amount, for the two positions of the polarising nicol, according as the major or the minor axis of the ellipse is in the plane of incidence, the vibration in the plane of incidence being the one retarded and reduced in amplitude by the act of reflexion.

The ratio of the semi-axes of the ellipse being called γ, and the observed azimuths a_1 and a_2, the ratio of the amplitudes of the vibrations in the plane of, and perpendicular to, the plane of incidence after reflexion by the metallic plate (the amplitudes in the incident beam being equal), or, in other words, the principal azimuth δ, is determined by the equations—

$$\left.\begin{aligned} \tan a_1 &= \gamma\delta \\ \tan a_2 &= \frac{\delta}{\gamma} \end{aligned}\right\}$$

which give $\tan a_1 \tan a_2 = \delta^2$.

The table contains the determinations made in this way with the silver plate in air, water, and carbon tetrachloride; the position of the principal section of the polarising nicol, with reference to the plane of incidence, being stated in the first column, and the mean values of the principal incidences, and the values of the principal azimuths, calculated by the above-mentioned formula, in the last line of each portion of the table—

		Red light. P.I.	Red light. A.	Yellow light. P.I.	Yellow light. A.	Blue light. P.I.	Blue light. A.
		° ′	° ′	° ′	° ′	° ′	° ′
Silver in air.	pll.	76 03	36 11	75 20	35 52	72 07	31 44
	ppd.	76 19	29 26	75 35	35 30	72 32	47 38
		76 11	32 43	75 27	35 41	72 19	39 28
Silver in water.	pll.	73 58	37 34	72 56	36 34	69 48	31 14
	ppd.	74 03	30 57	73 13	35 52	69 59	48 14
		74 01	34 11	73 05	36 13	69 53	39 29
Silver in carbon tetrachloride.	pll.	73 04	38 50	72 59	37 48	68 49	31 44
	ppd.	73 09	31 17	72 38	37 07	68 19	49 44
		73 07	34 58	72 47	37 27	68 34	40 30

The azimuths for silver in air and water with yellow light were so close to one another, that the mean was taken in these two cases as the principal azimuth.

The determinations of the reflexion constants of the silver plate made by the two different methods agree fairly well together, but are not in accordance with subsequent observations made with films of chemically deposited silver, or with M. Jamin's experiments ("Cours de Physique," edit. 1866, vol. iii, p. 693), the principal incidences being somewhat too high, and the principal azimuths having different values for light of different refrangibilities, instead of being nearly the same for all kinds of light.

The plate having been polished with rouge, and having, as has already been mentioned, a reddish tinge, due in all probability to minute particles of the powder having become embedded in its surface, it appeared possible that the difference in the values of the azimuths for the three kinds of light might be due to the rouge; the plate was therefore dismounted and well polished with putty powder, which was chosen as being nearly white, and then the measurements repeated in the usual manner.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Silver in air.	Plate at A.	78 26	44 02	75 0	44 11	71 28	42 56
	Plate at B.	74 33	43 41	74 14	43 16	71 39	43 04
	Mean value	76 29	43 51	74 37	43 22	71 33	43 0
Silver in water. I.	Plate at A.	76 18	44 27	72 38	44 54	67 15	43 10
	Plate at B.	71 37	43 46	71 22	43 20	68 23	43 05
	Mean value	73 57	44 06	72 0	44 07	67 49	43 07
Silver in water. II.	Plate at A.	75 53	43 59	73 02	44 37	66 27	43 51
	Plate at B.	71 53	43 59	71 59	43 46	67 42	43 41
	Mean value	73 53	43 59	72 30	44 11	67 04	43 46

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
Silver in carbon tetrachloride. I.	Plate at A.	74 09	44 26	72 05	43 31	65 20	45 25
	Plate at B.	70 43	44 12	71 12	43 09	67 33	43 21
	Mean value	72 26	44 19	71 38	43 20	66 26	44 23
Silver in carbon tetrachloride. II.	Plate at A.	75 11	44 34	72 13	43 56	67 10	45 44
	Plate at B.	70 36	44 09	71 10	44 05	67 51	43 37
	Mean value	72 53	44 21	71 41	44 0	67 30	44 40
Mean value.							
Silver in air.		76 29	43 51	74 37	43 22	71 33	43 0
Silver in water.		73 55	44 02	72 15	44 09	67 26	43 26
Silver in carbon tetrachloride.		72 39	44 20	71 39	43 40	66 58	44 31

The values of the principal incidences are nearly the same as when the plate had been cleaned with rouge, but the principal azimuths, in addition to being considerably higher, differ but little for the three kinds of light instead of increasing with the refrangibility of the light.

Films of silver chemically deposited on glass were prepared by Martin's process ("Ann. de Chimie," 4th Series, vol. xv, p. 94). Glass slips, similar to those used for mounting microscopic objects, were well cleaned, polished with rouge, washed with water, a little nitric acid poured over them, and then again washed and dried.

A rectangular frame (about 15 centims. by 7 centims.), formed of two glass rods, the ends of which were fixed into pieces of wood, was placed in a shallow earthenware dish partly filled with distilled water; the surfaces of the pieces of glass which were to be silvered were wetted with a mixture of equal parts of alcohol and potassium hydrate solution, and then the glass slips placed with their ends resting on the glass rods, and with their wetted surfaces downwards. There was just sufficient water in the dish for the lower surfaces of the glass slips to be in contact with it, whilst the upper remained dry.

The silvering solutions were mixed in another dish, care being taken that the depth of the silvering solution was the same as that of the water, and the frame with the slips resting on it, transferred from one dish to the other.

The room in which the silvering was carried on being very cold the action was slow, and till at least five minutes had elapsed there was hardly any deposit; the slips were removed successively after 8, 11, 14, and 18 minutes, well washed with water, and placed on edge to dry. The reflexion constants in air, water, and carbon tetrachloride were observed, and then after the area of the silver had been measured, and the slips dried and weighed, the silver was rubbed off with a damp cloth, and the glass again dried and weighed, and from the loss of weight and the area, the thickness of the films calculated. The

density of the silver being taken as 10·62, that being the value for silver, finally divided by precipitation, given in "Watts' Dictionary," vol. v, p. 277.

		Thickness.
Film I, removed after	8 minutes,	0·00004035 millim.
,, II, ,, ,,	11 ,,	0·00006848 ,,
,, III, ,, ,,	14 ,,	0·00007772 ,,
,, IV, ,, ,,	18 ,,	0·00008995 ,,

These values for the thickness of the silver films agree fairly well, being of the same order, with those determined by Quincke ("Pogg. Ann.," vol. cxxix, p. 177), who found that similar films varied from 0·0000039 millim. to 0·000075 millim.

Silver films in air.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
I.	Plate at A.	72 55	30 14	68 57	28 24	66 30	31 02
	Plate at B.	68 35	30 02	68 31	26 32	66 29	30 54
	Mean value	70 45	30 08	68 44	27 28	66 30	30 58
II.	Plate at A.	73 39	40 16	70 23	39 26	66 56	40 31
	Plate at B.	69 03	39 39	69 36	38 05	66 43	38 39
	Mean value	71 21	39 57	69 59	38 45	66 49	39 35
III.	Plate at A.	74 04	41 25	70 53	42 05	66 40	40 55
	Plate at B.	69 42	41 10	70 01	41 39	67 17	40 15
	Mean value	71 53	41 17	70 27	41 52	66 58	40 35
IV.	Plate at A.	74 32	41 21	71 0	41 39	66 36	40 35
	Plate at B.	69 35	41 08	70 37	41 17	67 19	41 0
	Mean value	72 03	41 14	70 48	41 28	66 57	40 47

Silver films in water.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
I.	Plate at A.	69 53	29 32	67 07	33 25	61 39	33 44
	Plate at B.	66 42	31 09	67 08	34 40	63 32	34 20
	Mean value	68 17	30 20	67 08	34 01	62 35	34 02
II.	Plate at A.	70 22	39 34	66 56	39 07	62 30	39 01
	Plate at B.	65 41	39 05	66 47	38 44	62 43	39 14
	Mean value	68 01	39 19	66 51	38 55	62 36	39 07
III.	Plate at A.	69 37	41 27	66 06	40 59	62 0	39 09
	Plate at B.	65 13	41 11	66 02	40 24	62 32	38 42
	Mean value	67 25	41 19	66 04	40 41	62 16	38 55
IV.	Plate at A.	70 34	41 02	66 41	41 01	62 44	39 12
	Plate at B.	65 23	40 26	66 0	41 11	63 12	39 14
	Mean value	67 58	40 44	66 20	41 06	62 58	39 13

Silver films in carbon tetrachloride.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
I.	Plate at A.	69 20	35 30	65 59	34 19	60 55	34 59
	Plate at B.	65 13	33 24	64 20	34 54	61 31	34 40
	Mean value	67 16	34 27	65 19	34 36	61 13	34 49
II.	Plate at A.	69 38	39 51	66 29	38 49	61 49	37 54
	Plate at B.	64 30	38 59	65 20	37 25	62 13	38 14
	Mean value	67 04	39 25	65 54	38 07	62 01	38 04
III.	Plate at A.	68 21	40 42	64 30	39 12	60 26	37 39
	Plate at B.	63 20	40 22	64 09	40 14	60 57	38 22
	Mean value	65 50	40 32	64 19	39 43	60 41	38 0
IV.	Plate at A.	68 54	41 20	65 05	40 0	61 05	38 21
	Plate at B.	63 56	40 24	64 46	39 45	61 46	40 50
	Mean value	66 25	40 52	64 55	39 52	61 25	39 35

Five similar glass slips were silvered in the same manner, being removed from the silvering solution after 7, 10, 13, 16, and 19 minutes, and the reflexion constants for air determined. The thickness of the films was not ascertained, as they were required for some other experiments, and were therefore kept.

Silver films in air.

		Red light. P.I.	Red light. P.A.	Yellow light. P.I.	Yellow light. P.A.	Blue light. P.I.	Blue light. P.A.
		° ′	° ′	° ′	° ′	° ′	° ′
V.	Plate at A.	73 0	30 25	69 37	31 32	66 48	33 11
	Plate at B.	68 27	28 10	70 02	30 52	66 24	32 17
	Mean value	70 43	29 17	69 49	31 12	66 36	32 44
VI.	Plate at A.	73 18	37 10	69 50	38 07	66 42	38 29
	Plate at B.	68 39	37 36	68 47	37 31	66 04	37 35
	Mean value	70 58	37 23	69 18	37 49	66 23	38 02
VII.	Plate at A.	74 06	41 24	70 49	41 14	67 32	40 0
	Plate at B.	69 53	40 49	70 10	40 47	66 32	40 04
	Mean value	72 0	41 06	70 30	41 0	67 02	40 02
VIII.	Plate at A.	74 43	41 20	71 25	41 54	67 41	41 02
	Plate at B.	70 13	41 25	70 51	40 24	66 56	40 52
	Mean value	72 28	41 22	71 08	41 09	67 18	40 57
IX.	Plate at A.	75 21	42 20	71 47	42 24	66 36	42 09
	Plate at B.	70 42	42 32	70 47	42 19	67 25	41 52
	Mean value	73 01	42 26	71 17	42 21	67 0	41 0

The table for the silver film in air shows that with the increase of the thickness of the film the principal incidence increases for all three kinds of light, but the increase is greatest with red and least with blue light. The principal azimuth also increases with the thickness, but does so more rapidly, the difference between the values for films I and II and between V and VI being very considerable, whilst the values for films II, III, and IV and VII, VIII, and IX lie close together.

With films II, V, and VI the azimuths increase from red to blue, whilst with the thicker films they decrease; film I, however, furnishes an exception, for, although very thin, the azimuths decrease from red to blue.

In this respect the thicker films behave like the silver plate cleaned with putty powder (p. 496), and the actual values of the azimuths in both cases are about the same; the principal incidences, however, are much lower with the films than with the plate. The difference in the values of the principal incidences may possibly be due to the effect produced on the plate by the pressure necessary to polish it.

When the films were in water and carbon tetrachloride, the principal incidences decreased to a slight extent with the increased thickness, but the variation was small in amount. The azimuths in the case of film I in water increased from red to blue, but in carbon tetrachloride, and with the other films in both liquids, the azimuths decreased from red to blue.

When the films were in contact with the liquids, the principal incidences were lower, except with film I, than in air, and the principal azimuths were also lower, instead of being higher, as had been the case with the gold and silver plates.

Quincke ("Pogg. Ann.," vol. cxxix, p. 177) made a large number of observations with silver films and red light, and found that the values of the principal incidences increased with the thickness, and tended to a constant value, and also that as soon as the thickness of the metal exceeded 0·00002 millim. there was but little further change in the value of the principal incidence.

In the short paper which appeared in the "Proc. Roy. Soc.," vol. 28, p. 242, it is stated that the numbers obtained by multiplying the tangents of the angles of principal incidence for red light and the gold plate in water and carbon bisulphide, by the refractive indices of these media, were somewhat higher than the tangents of the angles of principal incidence for air. This is also the case for the determinations made with the gold plate and yellow and blue light, and for those made with gold leaf, the silver plate, and the silver films with all three kinds of light. Thus for the silver plate cleaned with putty powder the values are as follow:—

	Red light.	Yellow light.	Blue light.
	° ′	° ′	° ′
Principal incidence in air observed	76 29	74 37	71 33
Calculated from observations made in water.....................	77 47	76 30	72 46
Calculated from observations made in carbon tetrachloride........	77 54	77 18	73 52

In the case of the silver films the difference between the observed and calculated values is greatest with the thinner films (amounting to nearly 4° for the thinnest, with red light) and diminishes as the films increase in thickness.

The values for the refractive index for gold were also calculated by the formula given by Lundquist ("Pogg. Ann.," vol. clii, p. 405), $\mu^2_A = \tan^2 A(1 - \sin^2 A . \sin^2 2\theta)$, and the values for the relative indices for water and carbon bisulphide multiplied by the refractive indices of these media, but the numbers thus obtained did not agree with those deduced from observations of the principal incidence and azimuth in air.

These experiments appear to show, first, that with glass in contact with media other than air, the tangent of the angle of polarisation is equal to the relative refractive index of the media.

Second, that the optical constants of a polished metallic plate depend, to a certain extent, on the substance with which it has been polished, and that this surface condition is a fairly permanent one, not being destroyed by contact with liquids or by a considerable amount of rubbing with a clean chamois leather.

Third, that when the plate is in contact with media other than air, the principal indices are lower, and the principal azimuths higher than in air; but that there is no simple relationship discoverable between the change in the values of the constants and the indices of the media.

Fourth, that with metallic films the principal incidence and the principal azimuth increase with the thickness of the film, and that therefore more than one layer of molecules is concerned in the act of reflexion.

The fact that with the increase of the thickness of the films the principal azimuth increases appears to show that the surface layers reflect light polarised in the plane of incidence more abundantly than light polarised perpendicularly to that plane, and that as the number of reflecting layers increases, the amount of light polarised perpendicularly to the plane of incidence which is reflected increases also; or, in other words, the light polarised perpendicularly to the plane of incidence penetrates to a greater depth than that polarised in the plane of incidence.

II. "On the *Trichophyton tonsurans* (the Fungus of Ringworm)." By George Thin, M.D. Communicated by Professor Huxley, Sec. R.S. Received February 19, 1881.*

(Abstract.)

When hairs affected with the *Trichophyton tonsurans* are cultivated in cells, the development of the spores on the sides of the hairs can, if it occurs, be observed *in situ* under the microscope. When the attempted cultivation takes place on the surface of a fluid in a test-glass, it is also possible, after maceration in solutions of potash, to decide whether the spores in the hairs have grown out from the surface of the hair, and to distinguish between a growth of adventitious fungi and the growth of the *Trichophyton*.

The paper gives an account of experiments made by the use of cells and test-glasses, which were kept at a temperature of between 92° and 98° F., but in a few instances at the ordinary room temperature. The *Trichophyton* remained sterile in cultivations attempted with a solution of phosphate of soda and tartrate of ammonia, with Cohn's fluid, milk, carrot infusion, turnip infusion, salt solution (0·75 per cent.), egg albumen, egg albumen and potash, and vitreous humour and potash. The only method by which it was grown was by moistening the hairs with vitreous humour. When moistened with vitreous humour, the spores on the sides of the hairs placed in cells were seen to grow into a mycelium, and free growth took place when the hairs were floated on the surface of this fluid in test-tubes. It did not grow in cells when the hairs were immersed in a large drop, nor in test-tubes when the hairs were kept at the bottom of the tube.

The growth observed consisted in a formation of mycelium, which sprouted from the spores in the hairs, and in the formation of spores in the newly-formed mycelium.

The successful cultivations were, with one exception, at the incubator temperature. In the exceptional instance the fungus grew at room temperature, but more feebly and slowly than at the incubator temperature.

It was shown by experiments in which *Aspergillus*, *Penicillium glaucum*, and other fungi grew around the hairs, whilst the spores of the *Trichophyton* remained sterile, that the latter is essentially distinct from the common fungi whose spores are present in the atmosphere.

The development of the spores by the only method found successful could not be relied on as certain in any given case. It was not found

* Towards the expenses of this research a grant was made by the British Medical Association on the recommendation of the Scientific Grants Committee of the Association.

successful in hairs that had been kept for a period of weeks folded in paper, nor in nineteen cultivations attempted with hairs taken from patients under treatment. The negative value of these latter experiments is diminished by the occasional failure with hairs freshly extracted from untreated cases.

The fact that the spores of the *Trichophyton* will not grow when immersed in vitreous humour, whilst they do grow when only moistened by it, explains why inflammatory exudation from the blood-vessels cures ringworm of the scalp.

III. "On *Bacterium decalvans:* an Organism associated with the Destruction of the Hair in Alopecia areata." By George Thin, M.D. Communicated by Professor Huxley, Sec. R.S. Received February 19, 1881.

(Abstract.)

The author having in several cases of Alopecia areata found bacteria adherent to the roots of extracted hairs, subjected hairs in six selected cases to processes designed to demonstrate the existence of organisms, should they be present, in the substance of the diseased hairs.

In five out of the six cases an object was observed in the hairs which he believes to be a bacterium. It was seen as a rounded or elongated spheroid, and was found frequently in pairs, the long diameter of the two spheroids forming a continuous straight line. Sometimes three were found in line, a delicate rod-shaped sheath enveloping the three. These bodies were, as was shown by the processes to which the hairs were subjected, neither oily particles nor crystals, and they could be distinguished from the granules always present in hairs. In all the cases their size and form were the same, and they had the refractive qualities of bacteria.

In hairs which were only slightly affected they were found between the inner root-sheath and the hair-shaft, and in small clusters on the hair-shaft beneath the cuticle of the hair. In hairs which were much diseased they were found in great numbers inside the cuticle of the hair, in the disintegrated hair substance.

Some hairs were found split into ribbon-like bands not far from the root and the organisms were found on the bands.

They were found only in the part of the hair which is under the surface of the skin, and most abundantly not far from the root.

In seven consecutive cases the disease was at once and definitely arrested by a treatment designed to destroy the vitality of any bacteria which might be present on the surface of the skin, and at the same

time to present a mechanical obstacle to their progress in growth from one hair follicle to another.*

IV. "On the Absorption of Pigment by Bacteria." By GEORGE THIN, M.D. Communicated by Professor HUXLEY, Sec. R.S. Received February 19, 1881.

Whilst occupied with cultivation of ringworm hairs in vitreous humour at a temperature of 92 to 98° F., I had occasion to observe in studying the hairs under the microscope, amongst the forms of bacteria which were invariably found in these cultivations, certain appearances that seem worthy of note.

For the present, I shall limit my remarks to one of these appearances.

It is known that certain fungi possess the property of taking up colouring matter from the medium in which they grow; and I have had occasion to observe in the *Trichophyton tonsurans* that both in man and in the horse the fungus may acquire a dark colour from absorbed pigment.

In the case of the horse, I have found the mycelial wall represented by an apparently empty dark tube; and I have found, at the same time, spores blackened with a coating of pigment.

I have found an analogous appearance in bacteria.

The bacteria found in these cultivations are seen in the transition forms of a spore or coccus, an elongating spore, rods, elongated rods, sometimes of great length, long rods, with the first appearance of a differentiation of the protoplasm into sporules, and finally as tubes full of spores or cocci.

These appearances have been now followed in several specific organisms, and notably, and first of all, in the *Bacillus anthracis*. They would seem to indicate the ordinary life-history of at least many bacteria.

I observed that frequently the preparations contained long bacteria rods which had taken up pigment from the hair. This pigment was often found at one end of a long rod, whilst towards the other end the rod was free from it; and in the part of the rod in which the pigment was found the spore formation could, in several instances, be

* As treatment which is destructive of bacteria would also arrest the development of a fungus, it is desirable to add that in none of these cases, nor in previous ones, was the author able to find the fungus described by Gruby, although a large number of hairs were examined. The examinations thus made were so exhaustive that he can only explain the alleged existence of a fungus in this disease by assuming that the distinction between Alopecia areata and some of the forms of ringworm has not been always kept in view.

seen to be more advanced than at the part which was free from pigment. The pigment was packed in the tubes around and between the spores; but, by focussing, it could be seen that the substance of the spore was free from it. The free spores and short rods were free from pigment.

The bacteria in which it was observed showed no other peculiarities, and were of about the same calibre as the rod bacteria usually observed.

The fact is noted as affording proof that bacteria can take up minute solid particles through their walls.

V. "On Toroidal Functions." By W. M. Hicks, M.A. St. John's College, Cambridge. Communicated by J. W. L. Glaisher, F.R.S. Received February 21, 1881.

(Abstract.)

This paper contains the development of a theory for functions which satisfy Laplace's equation, and are suitable for conditions given over the surface of a circular anchor ring, and which therefore seem important in the possibility of their application to the theory of vortex rings, as well as other physical problems. From the nature of the case, it will not be easy to give an intelligent and full abstract of the results without making it unduly long, but it may be possible to give some idea of its scope and the method of development.

Curvilinear co-ordinates are employed, the orthogonal surfaces being those formed by the revolution of a system of circles through two fixed points, and the circles orthogonal to them, whilst the third system are planes through the axis of revolution. Calling these v, u, w, it is shown that any potential function can be expanded in the form—

$$\phi=\sqrt{\cosh u-\cos v}\,\Sigma\Sigma\,(\mathrm{AP}_{m\,.\,n}+\mathrm{BQ}_{m\,.\,n})\cos(nv+\alpha)\cos(mw+\beta)$$

where $\mathrm{P}_{m.n}$, $\mathrm{Q}_{m.n}$ are particular integrals of a certain differential equation, and which are the toroidal functions whose discussion forms the principal part of the paper. They are in fact the same as spherical harmonics of the first and second kinds of imaginary argument, and of orders of the form $(2p+1)/2$. It is shown how the P can be expressed in terms of the first and second complete elliptic integrals F, E; and the Q in terms of the complementary integrals F′, E′. Several interesting results are arrived at, amongst others on relations between the P and Q functions, *e.g.*, between the zonal functions $(m-v)$ $\mathrm{P}_{n+1}\mathrm{Q}_n-\mathrm{P}_n\mathrm{Q}_{n+1}=\frac{\pi}{2}/(2n+1)$. The P_n serve for expansions in the space outside a tore, whilst the Q_n serve for space within.

Section ii is devoted to the consideration of zonal functions, *i.e.*, functions suitable when the conditions are symmetrical about an axis. Section iii deals with the general case, whilst in the last section illustrations of the use of the functions are given by application to several problems, such as the potentials of tores under the action of an electrified ring, or point of electricity, and the velocity potential when a tore moves parallel to itself in a fluid. Among the results obtained, which may be mentioned here, is the electrical capacity of an anchor ring. When the section of the ring is not very large compared with the central opening, a very close approximation is given by the formula—

$$q=\sqrt{R^2-r^2}\left\{\frac{F'}{F}+\frac{F'}{E}-\frac{E'}{E}\right\}$$

where $$k^2=2\frac{\sqrt{R^2-r^2}}{R+\sqrt{R^2-r^2}},$$

R, r being the radii of the circular axis, and generating circle of the ring respectively.

The approximation is so close that the formula only makes an error of about ·3 per cent. when r is so large as $\frac{1}{3}$ R.

If a tangent be drawn from the centre to the anchor ring, and a sphere be described with this for radius, the capacity of the tore measured in terms of that of the sphere is

$$\frac{F'}{F}+\frac{F'}{E}-\frac{E'}{E},$$

when R=10r this is ·36049,

when R=5r this is ·43405.

VI. "Microscopical Researches in High Power Definition. Preliminary Note on the Beaded Villi of Lepidoptera-Scales as seen with a Power of 3,000 Diameters." By Dr. Royston-Pigott, F.R.S. Received January 15, 1881.

In carrying out the investigation of the molecular structure of insect scales, under the finest attainable amplification, the discovery has been made that the striated surfaces of these scales, though appearing approximately beaded, are really covered with villi, chenille or velvet pile, terminating in a spherule.

The recognised object of these striæ regarded as corrugations is to give strength to a most delicate tissue, which are again supported by *cross striæ*. Upon these transverse striæ I have discovered villi erected

upon them by twos and threes, and summits consisting of a refracting spherule. This very difficult observation appears to open a new field of research. The object upon which these beaded villi were first detected is the scale of the *Vanessa Atalanta*, or Red Admiral.

March 10, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Conversion of Radiant Energy into Sonorous Vibrations." By WILLIAM HENRY PREECE. Communicated by the PRESIDENT. Received February 21, 1881.

Messrs. Graham Bell and Sumner Tainter* have shown that, under certain conditions, intense rays of light, if allowed to fall with periodic intermittence upon thin disks of almost every hard substance, will set up disturbances in those disks corresponding to this periodicity which result in sonorous vibrations. Mr. Bell has subsequently shown that such effects are not confined to hard substances, but that they can be produced by matter in a liquid form.

These discoveries have elicited a considerable amount of interest, and have led to the inquiry whether the sonorous effects are due, as the discoverers themselves surmised, to *light*, or as the President of the Royal Society, Professor Tyndall, and others have suggested to radiant *heat*.

Messrs. Bell and Tainter have partially answered this question by showing that the disturbances are not necessarily due to light, for they found that sheets of hard rubber or *ebonite*—a substance opaque to light—do not entirely cut off the sounds, but allow certain rays to pass through, which continue the effect. M. Mercadier, who has studied the subject with great care,† has shown that the effects are confined to the red and ultra-red rays. Moreover, Professor Tyndall has shown‡ that gases, such as sulphuric ether, which he had proved to be highly absorbent of heat rays, while they are transparent to

* American Association for the Advancement of Science. Boston, August 27, 1880.

† "Comptes Rendus," tome 91, p. 929.

‡ "Journal of the Society of Telegraph Engineers," vol. 9, p. 404.

light rays, are remarkably sensitive to this intermittent action. Dr. Tyndall has more recently read a paper before the Society* proving that these sonorous effects are a function of all gases and vapours absorbing radiant heat, and that the intensity of the sounds produced is a measure of this absorption.

The negative proof of Messrs. Bell and Tainter can be rendered positive if it can be shown that ebonite is *diathermanous*. The author provided himself with several sheets of different substances, and a sensitive radiometer. A standard candle and a lime-light were used as sources of energy, the former fixed four inches and the latter four feet from the radiometer, which was carefully screened from all disturbing influences but that of the source. The number of revolutions made by the radiometer per minute was counted, first without any screen, and then with each sheet successively interposed; and the average of several observations was taken. The following is the result, and the numbers indicate the relative diathermancy of the substances used to the source of light used:—

1. Experiments on Diathermancy.

Material.	Source of radiant energy.	
	Candle, 4 inches.	Lime-light, 4 feet.
Air	100	100
Ebonite, No. 1 (·4 millim.)	60	91
" " 2 (·9 millim.)	24·3	79·3
" " 3 "	24·3	79·3
" " 4 "	24·3	68·2
" " 5 "	24·3	68·2
" " 6 (·4 millim.)	0	9
India-rubber (native)	44·3	61·4
" (prepared)	60	54·5
" (vulcanized)	0	0
" (and ozokerit)	0	6·8
Ozokerit (·5 millim.)	0·14	9
Carbon (2 millims.)	0	
Carbonised paper	0	
Blue foolscap	0	4·5
White note paper	0	4·5
Thin foreign	0	
Tissue	0·6	

Ebonite was, however, proved to be very variable, and while some pieces proved to be as diathermanous as rocksalt, others of the same thickness were found to be quite athermanous.

Ebonite therefore, being sometimes diathermanous and opaque, it is clear that the sonorous vibrations of Bell and Tainter are the result of

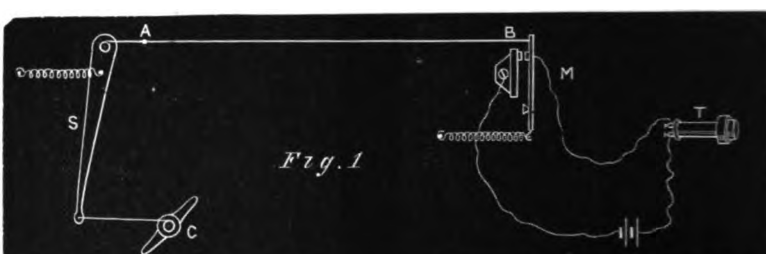
* "Proc. Roy. Soc.," vol. 31, p. 307.

disturbances produced by some thermic action rather than by any luminous effect. Several other experiments made by them confirm this conclusion, notably those made upon crystals of sulphate of copper, a substance which Mr. Crookes has shown to be highly opaque to rays of low refrangibility.*

Now, the questions arise, is this thermic action expansion and contraction of the mass due to its absorption of heat? Or is it a disturbance due to molecular pressure similar to that which produces the rotation of the radiometer? Or is it due to some other cause?

The argument against the first assumption when applied to hard disks, is that *time* is a material element in such actions, and that the rate of cooling of warmed diaphragms is too slow to admit of such effects. Lord Rayleigh† has questioned the validity of this argument, and has shown that if the radiating power of the body experimented on were sufficiently high a slow rate of cooling would be favourable to rapid fluctuations of temperature. It became desirable to test this point experimentally. The following apparatus was constructed for the purpose.

AB is a thin strip or wire 6 centims. in length of the substance to be examined, fixed to a platinum "make and break" M, and adjusted to a lever S, round whose axis is fastened a silk thread, the end of which is attached to the strip or wire at A, and whose position could



be adjusted by a screw C. Any variation of tension due to expansion or contraction of the wire would produce intermission in the electric currents passing through the telephone T, which if periodically produced would result in sonorous vibrations in the telephone. Heat from various sources and at various distances from bright lime-light to dull heat from hot metallic surfaces, was allowed to fall through rotating vanes intermittently on AB; but notwithstanding every precaution, and the many materials used, not more than six interruptions per second could be produced, although the system was beautifully sensitive to the smallest changes of temperature.

* "Phil. Trans.," vol. 169, § 268.

† "Nature," January 20, 1881.

The best effect was obtained when AB was a thin ebonite wire about ·5 millim. in diameter, and 6 centims. in length.

It was evident from these experiments that the sonorous effects of hard disks could not be explained by the change of volume due to the impact of heat rays, and their absorption by the mass of the disk.*

Is the action then due to molecular pressure similar to that which produces the rotation of the radiometer?

It is quite true that the radiometer effect is one visible only in very high exhaustions, but Mr. Crookes ("Phil. Trans.," vol. 169, § 220) detected "the existence of molecular pressure when radiation falls on a black surface in air of normal density."

Whenever radiant energy falls on an absorbent surface in air, such as a disk of blackened wood, its wave-length is degraded or lowered, and it is converted in thermometric heat. The molecules of air striking this warmed surface acquire heat, and move away from it with increased velocity, and as action and reaction are always alike in moving away, they give the body a "kick." Since there is no such action on the other side of the disk, there is a difference of pressure between the two sides, which gives it a tendency to move away from the source of energy. The effect is very much smaller in air at ordinary pressures than in air at a very low vacuum, because in the former case, the mean free path of the molecules is very small, and the rebounding molecules help to keep back the more slowly approaching molecules. Nevertheless, molecular pressure is experienced, and if of sufficient magnitude and rapidity, it ought to produce sonorous vibrations. It seemed probable that the element of time does not enter here so prominently as in the previous case, for the radiometer effect is a mere surface action of the disk, and not one affecting its mass. Hence it was hoped that the retarding effects would be eliminated. If the sonorous action, therefore, be due to a radiometer action, a difference of effect would be observed if the side

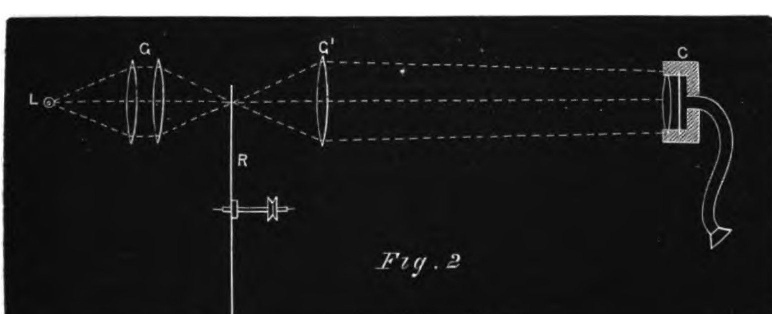
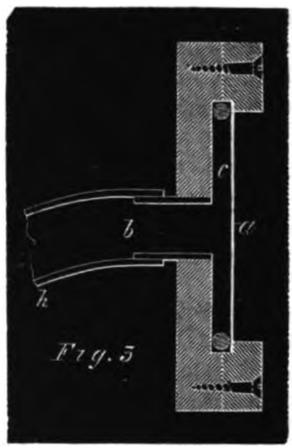


Fig. 2

* This conclusion was confirmed by subsequent experiments, notably by Experiments 7 and 41.

of a disk *exposed* to the source of energy, be either blackened by lamp-black or camphor carbon, or if it be polished or whitened.

An apparatus was constructed similar in principle to that described by Messrs. Bell and Tainter. The source of light (L) was an oxy-hydrogen lime-light. The rotating disk (R) was of zinc perforated with holes, which could be noiselessly rotated, so as to obtain 1,000 intermissions per second. Glass lenses (G) were employed to focus the light upon the perforations of the rotating disk, and another (G′), to render the rays parallel on the other side of the disk. A mahogany



case or cup (C) to retain the disks to be experimented upon was constructed as shown in section in fig. 3, and fixed 400 centims. from the source L; *a*, being the disk 5 centims. in diameter, clamped on by screws; *b*, a brass tube, to which the india-rubber hearing tube (*h*) was fixed; *c*, a circular air space behind the disk 6 centims. in diameter, and 3 millims. to 5 millims deep. Cavities of various dimensions and forms, spherical, conical, and trumpet-shaped, were tried, but the ones described were those which gave the best effects.

Experiment 2.—An ebonite disk well blackened on one side when exposed to the intermittent rays was found to produce sounds, which Professor Hughes estimated to be about 20 as compared with his sonometer scale.*

* Professor Hughes' Sonometer ("Proc. Roy. Soc.," vol., 29, p. 56) proves an excellent apparatus to estimate the relative intensity of low sounds, for by shifting the induction coil from one side to the other, we can pass from an absolute zero, or pure silence, to a limit of sound of considerable intensity. The scale divides this space passed over into 100 equal parts, any number of which give a fairly approximate value of the sounds compared. The ear has to determine the equality of the

3.* A similar ebonite disk equally well whitened, gave slightly less intense sounds estimated at 18.

4. A zinc disk blackened gave sounds =8.

5. A similar disk polished gave only sounds =2.

6. A mica disk blackened gave scarcely any sounds at all.

7. A clean mica disk gave sounds =5.

These effects were produced many times, and on different occasions, and they were so unsatisfactory as to throw doubts on the accuracy of the radiometer explanation. They were not so decided as theory led one to anticipate. The effects produced by the zinc disk, though very weak, favoured the theory; those given by the mica disk completely refuted it; while those given by the ebonite disks were almost of a neutral character.

It was, however, thought that if D be the disk (fig. 4), and C the

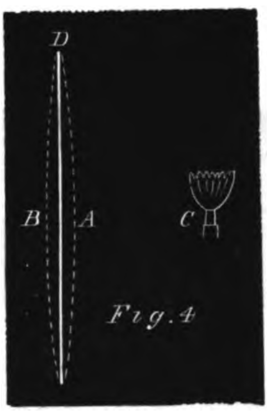


Fig. 4

source of light, then if the excursions of the disks to and fro were due to expansion from the absorption of heat, it would first bulge towards A, since the side towards the source of light would expand first. If, on the other hand, it were due to the radiometer effect, it would first bulge towards B.

8. An extremely delicate electrical contact arrangement was constructed to determine this by means of a telephone, which recorded the excursions to and fro of the disk; but the result was sometimes in one direction, and sometimes in the other. Moreover, the effect was slow, and we failed to obtain more than five distinct vibrations per second.

This result raised the question whether in Bell and Tainter's experiments *the disks vibrated at all.*

intensity of the two sounds, *i.e.*, of the sonometer and the source investigated, and the value of the latter is given in terms of the scale of the former.

* The consecutive figures indicate the number of the experiments.

9. A delicate microphone was fixed in various ways on the case, fig. 3. Although the sounds emitted in the hearing tube were as intense as indicated in Experiment 1, scarcely any perceptible effect was detected on the microphone. Had the disk sensibly vibrated, its vibrations must have been taken up by the case. A microphone never fails to take up and magnify the minutest mechanical disturbances.

It was thus evident that the disk did not play a prime part in this phenomenon, but it appeared as Professor Hughes suggested, that the result might be due wholly to an expansion and contraction of the air contained in the air space *c*, fig. 3.

An entirely new set of cases and disks were now constructed, but of the same dimensions.

10. With a new clean case and an ebonite disk, the sonorous effects were feeble, viz., 15 on the sonometer scale.

11. If, however, a lens (*d*) were placed close in front of the ebonite disk *a*, (fig. 5), the sonorous effects were magnified considerably, rising at once to about 40.

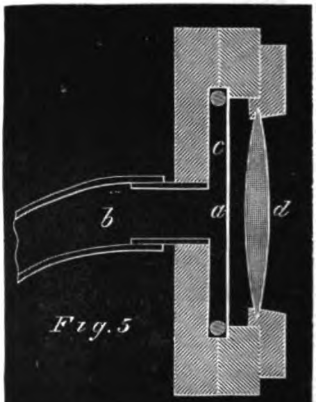


Fig. 5

12. A case was constructed similar to that shown in fig. 5, but with a tube at the side, as well as one at the back, communicating with the air cavity in front of the disk, as well as that behind, and whether one listened at one place or the other, the effect was equally good, indicating that the results were most probably due to the expansions of the contained air.

13. When the one tube was stopped and opened by a cork while the other tube was used for listening, the quality of the sounds varied more than the intensity, but there was distinct evidence of variation.

14. The ebonite disk of this case was now fitted with an extremely delicate microphone, which in this case gave good indications upon the telephone, but whether the vibrations were the results of the

vibrations of the disk itself, or of the air in which the microphone was placed, was doubtful.

15. If the lens *d*, fig. 5, were removed, and the disk left supported without any air cavity, either behind or in front of it, *no perceptible sound could be obtained*, proving that the effects were really due to the vibrations of the confined air, and *not to those of the disk.* It was thèrefore determined to dispense with the disk altogether.

16. Case (No. 1) similar to fig. 5 was taken, and the disk removed, but the lens remaining, the sonorous effects were *nil.*

17. Another case (No. 2), also similar to fig. 5, was taken under similar circumstances, *i.e.*, *without the disk*, but the effects were very loud—60; in fact, the best results which had yet been obtained. Now, the only difference between the one case or cup and the other, was that the one *was blacked in the interior*, and the other was not.

18. Hence Case No. 1 was again taken without the disk, and though when clean it gave no effect, when its interior was blacked by camphor smoke, it gave sounds as strong as those in Experiment 17, viz., 60. It was thus evident that the sonorous effects were materially assisted by coating the sides of the containing vessel with a highly absorbent substance, such as the carbon deposited by burning camphor. It remained to be seen how far the lens played a part in this phenomenon.

19. The lens was now removed from the front of the case, and it was replaced by a moveable glass plate (1·5 millims. thick) (*e*) fig. 6; the sounds were the same, but they gradually ceased on gradually uncovering the front opening of the case, so as to give the air room to expand.

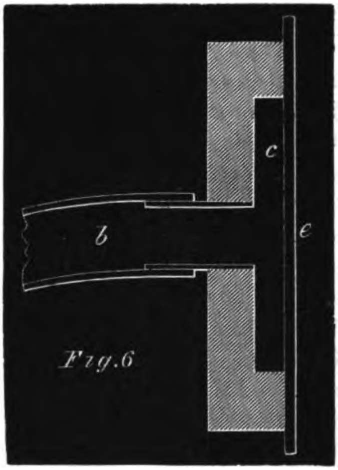


Fig. 6

20. The glass plate *e* was replaced by a heavy rigid plate of rock-salt 13 millims. thick, and the sounds were equally loud, namely, =60.

21. The plate *e* was replaced by white note paper. The sounds were very faint but perceptible.

22. It was replaced by thin cardboard, and the effect was *nil*.

23. All these effects were produced equally well, whether the cases were placed at C, fig. 2, 400 centims. from the source of light, or 16 centims. from the rotating disk R, but in the latter case their intensity was of course always increased.

Hence it is abundantly evident that these sonorous vibrations are due to the motions of the contained air, and not to those of the disk; that they are actually improved by the removal of the disk, that their production is materially assisted by lining the surface of the containing space with an absorbent substance, that they are dependent on the heat rays that pass through, and that they disappear when the rays are cut off from the air cavity by an athermanous diaphragm.

24. Dr. Tyndall having shown in the paper already referred to, that water vapour responded actively to these intermittent actions, a clean empty one-ounce glass flask was taken and exposed to the intermittent beams. No sound was produced.

25. It was then filled with water vapour by pouring a small quantity of water into it, and warming it in a flame, fair sounds of an intensity 25 were the result.

26. The flask was filled with the dense smoke from burning camphor, and the sounds were intensified considerably.

27. The case (fig. 6) was taken and a glass plate 1·5 millims. thick fixed in front of it as before.

a. When the glass was dry, sounds were 20.

b. When the glass was wetted on the inside, sounds were 25.

28. Another clear one-ounce glass flask was taken.

a. When clear, sounds = 0.

b. When filled with tobacco smoke, sounds = 5.

c. When filled with heavy camphor smoke, sounds = 30.

29. One side of the flask was blackened on the outside, the other side remaining clear.

a. On exposing the clear side to the light fair sounds 25 were obtained.

b. On exposing the blackened side, *no sounds were produced.*

30. The flask was blackened *in the interior* on one side only.

a. When the blackened side was near the source, sounds = 25.

b. When it was away from the source, sounds = 30.

c. When the flask was cleaned, all sounds disappeared.

31. A thin glass plate was now blackened on one side and placed in front of the case, fig. 6.

a. When the black surface was outside, *no sounds were obtained.*

b. When the black surface was inside, good sounds, 30, were the result.

c. When the glass was cleaned, the sounds became = 50.

32. An ebonite plate was similarly treated.

a. When the blackened surface was outside, sounds = 15 were obtained.

b. When the blackened surface was inside, the sounds were = 3. This being an anomalous result, several experiments were now made to test the behaviour of opaque and transparent bodies, when used as disks, for while in the previous experiments the effect was greatest when the blackened surface faced the interior, here we find the opposite result produced, viz., the greatest effect was produced when the blackened surface was on the exterior.

33. Another ebonite disk 0·6 millim. thick was made dull on both sides by rubbing it with emery paper. It was fixed in case (No. 2), fig. 5, without the lens.

a. It gave sounds = 15.

b. It was now blacked on one side, and when that side was turned to the source, it gave sounds = 30.

c. The unblackened side was turned to the source, and it gave sounds = 15.

34. A thin glass plate (1·5 millims. thick)—

a. When clear, gave sounds = 50.

b. When blackened *thinly* on one side, and that side turned to the source, it gave sounds = 20.

c. When blackened thickly on one side and thinly on the other—

1. Thick side to light, sounds = 0.
2. Thin side to light, sounds = 20.

35. A glass plate (3 millims. thick) was blackened on one side.

a. Black outside, sounds = 2.

b. „ inside, „ = 30.

36. A clean mica plate (1·2 millims. thick)—

a. Gave sounds = 30.

b. When blacked on one side.

1. Black outside, sounds = 10.
2. „ inside, „ = 30.

37. Thin copper foil blacked on either side, sounds were *nil*.

38. A copper disk (0·2 millim. thick) blacked on one side.

a. Black outside, sounds = 3.

b. „ inside, „ = 0.

39. Zinc foil blacked on one side.

a. Black outside, sounds = 10.

b. „ inside, „ = 3.

40. Zinc disk (0·7 millim. thick) blacked on one side.

a. Black outside, sounds = 10.

b. „ inside, „ = 3.

It thus appears that transparent bodies behave in an opposite way to opaque bodies. Glass and mica can be rendered athermanous and silent by making the carbon deposit sufficiently thick. Zinc, copper, and ebonite can produce sonorous effects by a proper disposition of carbon. The effect in these latter cases may be due either to molecular pressure, in fact, to a radiometer effect, though very feeble in intensity; or it may be the result of conduction through the mass of the diaphragm, that is, radiant heat is reduced to thermometric heat by absorption by the carbon deposit on the *outside* of the disk, this heat is transmitted through the disk and radiated to the absorbent gases in the interior.

41. To test these questions, a delicate microphone was fixed on a zinc disk (0·7 millim. thick) without any air-cavity in front of or behind it.

a. The disk was blackened; no measurable trace of sound was perceptible.

b. The disk was cleaned, with precisely the same result.

42. The zinc disk was now replaced on the case, fig. 5, with the air-cavity, and sounds = 10 were emitted as before.

a. The zinc was neatly covered with white paper—a highly athermanous substance; the sounds were *nil.*

b. The paper was blackened; the sounds were still *nil.*

c. The zinc was covered with clear mica; faint but unmeasurable sounds were heard.

d. The mica was blackened; no perceptible sounds were obtained.

e. The zinc disk was blackened slightly in the usual way; sounds = 10 were obtained.

f. The zinc was *thickly* blackened; all sounds disappeared.

43. Ebonite disks of different thicknesses were used, and layers of carbon of different thicknesses were deposited on the thinnest of them. The sounds became fainter and fainter as the thickness of the layer of carbon and the thickness of the ebonite increased, until they disappeared altogether.

These experiments fully establish the inference that the effect is one of conduction, and that the blackened surface of an opaque body like zinc acts as though the source of heat were transferred to the outside surface of the disk.

44. Tubes of various sizes and dimensions were now tried, to confirm Messrs. Bell and Tainter's observations on tubes. They invariably gave out satisfactory sounds when the intermittent rays were directed

into the interior of the tubes, which were always considerably intensified by blackening their interiors, and closing the open end with a glass plate.

45. Since the sounds varied considerably in intensity, according to the number of intermittences per second transmitted, white and ruby glass plates were used, and it was found that the maximum effect (60) was produced when the note corresponding to the intermittences was—

With white glass ...... $d' = 297$ vibr. per sec.
With ruby ,, ... $b = 247$,, ,,

This showed that the element of time was a function of the amount of radiant energy transmitted through the plate.

46. The two cases, which possessed air-cavities of different dimensions, were tried with white glass:

Case 1 gave $b = 247$ vibr. per sec.
Case 2 ,, $d' = 297$,, ,,

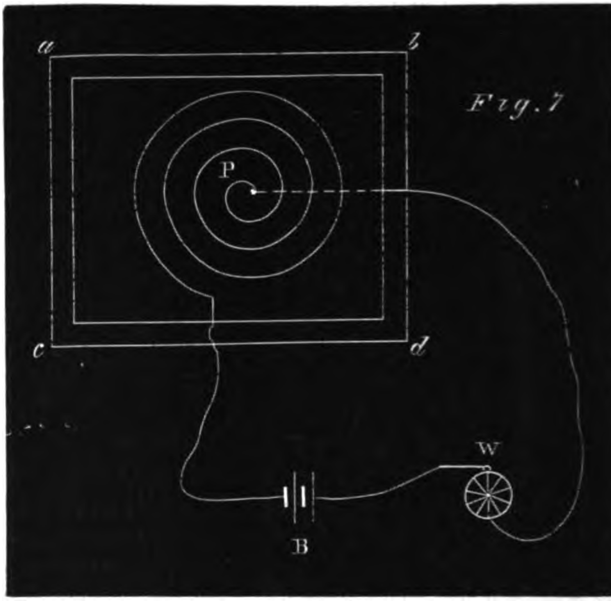
Hence it is evident that there is a time element, and that the loudness of the note emitted depends upon the rapidity with which the contained air not only absorbs the degraded energy, but upon the rapidity with which it gives up its heat to the sides of the case and the exits open to it. Though the pitch of the maximum note varied with the cavity and the amount of radiant heat transmitted, its quality never varied, notwithstanding the great diversity of materials used as diaphragms.

47. Since these sonorous effects are due to the expansions of absorbent gases under the influence of heat, and since wires are heated by the transference of electric currents through them, it seemed possible that if we inclosed a spiral of fine platinum wire P (fig. 7) in a dark cavity, *abcd*, well blacked on the inside, and sent through it, by means of the wheel-break W, rapid intermittent currents of electricity from the battery B, heat would be radiated, the air would expand, and sounds would result. This was done, and the sounds produced were excellent. In fact, with four bichromate cells, sounds more intense than any previously observed were obtained.

Furthermore it was evident that if the wheel-break (W) were replaced by a good microphone transmitter, articulate speech should be heard in the case *abcd*. This was done, and an excellent telephone receiver was the consequence, by means of which speech was perfectly reproduced.

The explanation of these remarkable phenomena is now abundantly clear.

It is purely an effect of radiant heat, and it is essentially one due to the changes of volume in vapours and gases produced by the degradation and absorption of this heat in a confined space. The disks in Bell



and Tainter's experiments must be diathermanous, and the better their character in this respect the greater the effect; remove them, and the effect is greater still. Messrs. Bell and Tainter* showed that the sounds maintained their *timbre* and pitch notwithstanding variation in the substance of the disk, and M. Mercadier found that a split or cracked plate acted as well as when it was whole. These facts are consistent with the expansion of the contained air, but not with any mechanical disturbance of the disks. Moreover, M. Mercadier showed that the effect was improved by lampblack, but he applied it in the wrong place.

The disks may, and perhaps do under certain conditions, vibrate, but this vibration is feeble and quite a secondary action. The sides of the containing vessel must possess the power to reduce the incident rays to thermometric heat, and impart it to the vapour they confine, and the more their power in this respect, as when blackened by carbon, the greater the effect. The back of the disk may alone act in this respect. Cigars, chips of wood, smoke, or any absorbent surfaces placed inside a closed transparent vessel will, by first absorbing and then radiating heat rays to the confined gas, emit sonorous vibrations.

The heat is dissipated in the energy of sonorous vibrations. In all cases, time enters as an element, and the maximum effect depends on

* "Journal of Society of Telegraph Engineers," December 8, 1880.

the diathermancy of the exposed side of the cavity, on its dimensions and surfaces, and on the absorbent character of the contained gas.

The remarkable property which deposited carbon possesses of reducing radiant energy to thermometric heat is strikingly shown by these experiments, and it suggests an important field for inquiry for those who are working in the region of radiant heat.

It is only necessary to add that, in carrying out these experiments, I have had the benefit of the presence and advice of Professor Hughes, and the inestimable advantage of the great mechanical skill, philosophical character, and experimental ability of Mr. Stroh, without whom, in fact, the inquiry could not have been accomplished.

Additional Note. Received March 14, 1881.

Professor Stokes has suggested that the action is due not to the expansion of the contained vapour through its absorption of thermometric heat generated on the lamp-black surface, but to the *contact* of the air molecules with this surface. It is clear that when the carbon is warmed, the rapidly moving air molecules which strike it and bound off have their retreating motion increased in velocity. This increased velocity means increased pressure, which in its turn produces increased volume, and this when intermittent, produces sonorous vibrations. This explanation is quite in accordance with the observed facts, for the difference of the intensity of the sounds emitted by dry air and air charged with absorbent vapours is very much less, when a lamp-black surface is used, than was anticipated. Dry air gives excellent results with lamp-black, but is silent without it. Indeed it leads one to conceive that as charging the air with heavy smoke produces the same effect as coating the containing surface with lamp-black, each particle of smoke becomes a warmed surface from which the colliding air molecules, whose dimensions are infinitely smaller, retreat with increased velocity. Moreover, if we conceive the smoke particles replaced by the compound molecules of absorbent vapours such as sulphuric ether, olefiant gas, or ammonia, and if we assume the dimensions of these molecules greater than those of the air, we have an explanation not only of the absorbent power of these gases, but of the reasons for their behaviour in converting radiant energy into sonorous vibrations. It is clear that if the compound molecule act as a smoke particle, the radiant energy becomes degraded into thermometric heat, for the motion of the ether is transferred to the motion of the molecule. The minute air molecules move unaffected in the undulating ether, but the larger compound molecules of the absorbent gas take up the waves of the ether, and assume that form of motion which is known as thermometric heat. Hence, the greater the number of molecules and the larger their dimensions, the greater the absorption

of heat and the more the production of sonorous vibrations, for a greater number of air atoms will collide with warmed surfaces in a given time. That this assumption is justified is proved by the fact that this absorption of radiant energy renders the particles of dissociated nitrite of amyl and other vapours visible as well as warm, and therefore they can assume dimensions that are comparable with the particles of smoke.

II. "On the Limit of the Liquid State." By J. B. HANNAY, F.R.S.E. Communicated by Professor G. G. STOKES, Sec. R.S. Received February 22, 1881.

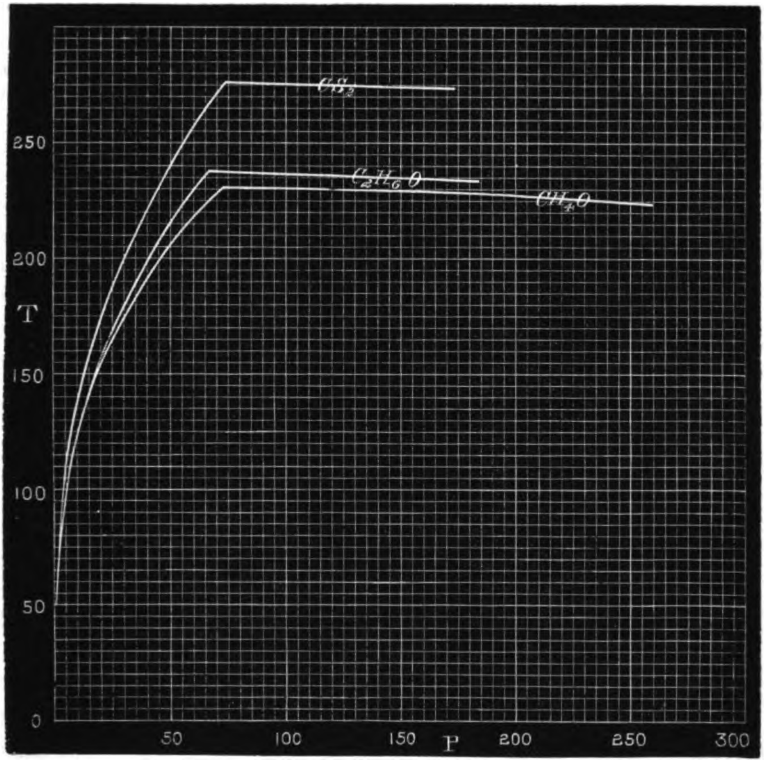
(Abstract.)

In this paper the author gives an extended account of the work undertaken to determine whether the liquid state extends above the critical temperature, or whether it is bounded by an isothermal line passing through the critical point, as had been indicated in a former paper. A large apparatus was constructed, with several improvements before described, details of construction being given in the full paper. It was found that manometers with small bores gave higher readings than those with larger internal diameter, so the manometers used were of the largest size compatible with the strength required to resist the pressure. The thermometers were carefully prepared by heating and cooling, and compared with the standard at Kew. All the usual precautions were taken to obtain accurate numbers. The critical temperature and pressure of the liquids were first determined accurately, and then a quantity of a gas insoluble in the liquid was compressed over the liquid, and the critical temperature again determined under increased pressure. When the densities of the two bodies, *e.g.*, alcohol and hydrogen, are far apart from each other, the gas shows no effect in lowering the critical temperature, as is the case with carbon dioxide and air, whose densities approach much nearer, but simply acts as a spring against which the upper surface of the alcohol bears, thus having a surface free for observation at pressures far above that of the vapour of the liquid. When the liquid passes the critical temperature at any pressure the meniscus is lost, and the fluid freely diffuses into the superincumbent gas, but this does not occur at temperatures below the critical, except where very high pressure has made the gas appreciably soluble in the liquid. Thus the curve of vapour tension, that is, the curve representing the temperatures at which a given pressure will produce liquefaction suddenly becomes isothermal at the critical point, and passes along the co-ordinate denoting the critical temperature.

As surface tension is the only property by which the liquid state

can be known, the capillary height of the liquid at various temperatures was next determined. The capillary height becomes zero at the critical temperature, and this is the case whether the pressure is the critical pressure or a higher one. Curves indicating the loss of capillarity with rise of temperature are given, and the observations repeated at higher pressures, the pressures being obtained by compressing hydrogen over the liquid. The surface tension is lowered a little by the action of the compressed hydrogen, but the change of

Fig. 1.

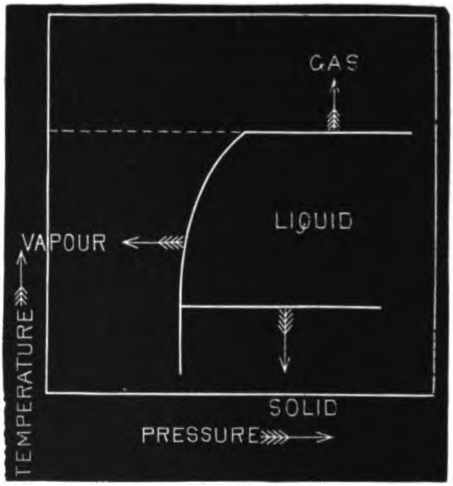


capillarity follows that of the liquid alone very closely, and the capillarity sinks to zero a few degrees below the critical temperature. Nitrogen may be substituted for hydrogen with the same results, and various other liquids being used—carbon bisulphide, carbon tetrachloride, and methyl alcohol. Curves are given, showing their behaviour under high pressures, as in the case of alcohol. In no case could any of the properties of the liquid state be found to exist above the critical temperature. The paper concludes: "Three

curves have been drawn to show the slight depression of the critical temperature with increase of pressure, and these lines have been continued down the curve of vapour pressure to show the break at the critical point." This will be clearly seen in the annexed fig. 1.

The consideration of these results yields a novel mode of looking at the states of matter which I have shown at fig. 2. From this, it appears we might classify matter under four states: First, the gaseous, which exists from the highest temperatures down to an isothermal passing through the critical point, and depending entirely upon temperature or molecular velocity. Second, the vaporous, bounded upon the upper side by the gaseous state, and on the lower by absolute zero, and depending entirely upon the length of the mean free path, because shortening of the mean free path alters the state. Third, the liquid state, bounded upon the upper side by the gaseous, and on the lower by the solid or absolute zero. Fourth, the solid state.

FIG. 2.



The gaseous state is the only one which is not affected by pressure alone, or in which the molecular velocity is so high that the collisions cause a rebound of sufficient energy to prevent grouping at any pressure. Another distinction between the gaseous and vaporous states is, that the former is capable of acting as a solvent of solids, while the latter has not that power.

The two conclusions arrived at are:—

"1st. The liquid state has a limit which is an isothermal passing through the critical point.

"2nd. The vaporous state can be clearly defined as a distinct state of matter."

III. "On the Diastase of Kôji." By R. W. Atkinson, B.Sc. (Lond.), Professor of Analytical and Applied Chemistry in the University of Tôkiô, Japan. Communicated by Professor A. W. Williamson, For. Sec. R.S. Received March 3, 1881.

(Abstract.)

The paper contains the results of an investigation into the nature of the material used in Japan for converting starch into sugar in the brewing operations. This substance "kôji" is prepared from steamed rice by allowing the spores of a fungus, mixed with the grain, to vegetate over the surface. Details of the manufacture are given, and it is shown that the rice suffers a loss of 11 per cent., calculated upon the substance dried at 100° C. At the same time a great evolution of heat occurs.

A solution of the soluble portion of the "kôji" thus prepared is shown to possess properties analogous to those of malt-extract, although differing from it in some important respects. It rapidly inverts cane-sugar and hydrates maltose and dextrin. It liquefies starch-paste, forming at first maltose and dextrin, but giving as ultimate products dextrose and dextrin. Curves accompany the paper showing the action of the extract of "kôji" upon starch-paste at different temperatures.

The paper concludes with an examination of the change which the rice grain undergoes by the growth of the mycelium of the fungus, and it is pointed out that the principal effect produced by the growing plant is to render the insoluble albuminoids previously existing in the rice soluble.

March 17, 1881.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

Professor J. Emerson Reynolds was admitted into the Society.

The following Papers were read :—

I. "On the Electrical Resistance of Thin Liquid Films, with a Revision of Newton's Table of Colours." By A. W. REINOLD, M.A., Professor of Physics in the Royal Naval College, Greenwich, and A. W. RÜCKER, M.A., Professor of Physics in the Yorkshire College, Leeds. Communicated by Professor W. GRYLLS ADAMS, F.R.S. Received March 3, 1881.

(Abstract.)

The authors have made numerous measurements of the electrical resistance of cylindrical liquid films. Their object was to determine whether a thinning film gave evidence by change in its specific resistance of an approach to a thickness equal to twice the radius of molecular attraction, and also to devise a method of finding the amount of water which might be absorbed by or evaporated from it. This change of constitution had been neglected by previous observers of the properties of films.

The thickness of the films was determined from their colour. This necessitated a revision of Newton's table of colours, which was carried out partly by observations on Newton's rings, partly by more than 2,000 observations on the films themselves. Two simultaneous but independent measures of the thickness of the film were made by observing two portions illuminated by light incident at different angles. In 84 per cent. of the measures made during the final series of experiments described in the paper, the difference between the two values of the thickness thus obtained did not exceed 2 per cent. of its value.

The films were enclosed in a glass case in which they could be formed without admitting any external air. Elaborate precautions were taken to maintain the aqueous vapour within the case at the tension proper to that in contact with the soap solution.

The resistance of the films was measured by piercing them with

gold wires which were in connexion with the opposite pairs of quadrants of a Thomson's electrometer. The resistance of the film between the needles was calculated by comparing the deflection caused by the difference of potential of the two wires when a current was passing through the film, with that produced by the difference of potential above and below a known resistance placed in the same circuit.

A novel method, the same in principle with the above, was also used to determine the specific resistance of the liquids from which the films were formed. This was deduced from the difference of potential of two platinum wires cemented into a glass tube in which the liquid was contained. As these were at some distance from the electrodes, errors due to polarisation were got rid of. The results of some test experiments made on solutions of sulphuric acid agreed with those of Kohlrausch to within 0·7 per cent.

The authors conclude that their experiments show that the specific resistance of a soap film thicker than $3{\cdot}74 \times 10^{-5}$ centims. (the least thickness at which trustworthy observations were made) is independent of the thickness, and is equal to that of the liquid from which it is formed.

They have, therefore, detected no indication of an approach to a thickness equal to the diameter of molecular attraction, and this leads to the deduction that its magnitude must either be less than is supposed by Quincke ($0{\cdot}5 \times 10^{-5}$ centims.), or that the mean specific resistance of the surface layer, the thickness of which is equal to that magnitude, does not differ by 17 per cent. from that of the liquid in mass.

They have further found that soap films, even in an enclosed space, may, if the precautions above referred to are not taken, readily lose 23 out of the 57·7 volumes of water contained in every 100 volumes of solution, and their experiments show that this quantity may probably be largely exceeded. They think, therefore, that in all accurate observations on soap films these profound modifications of constitution must either be prevented or measured by a method similar to their own. They criticise from this point of view the observations of Plateau* and Lüdtge,† and conclude by pointing out the extreme sensitiveness of the electrical method of investigation.

II. "Molecular Electro-Magnetic Induction." By Professor D. E. Hughes, F.R.S. Received March 7, 1881.

The induction currents balance which I had the honour of bringing before the notice of the Royal Society‡ showed how extremely sen-

* "Statique des Liquides," (1873), vol. i, p. 210.

† "Pogg. Ann.," (1870), vol. cxxxvii, p. 620.

‡ "Proc. Roy. Soc.," vol. 29, p. 56.

sitive it was to the slightest molecular change in the composition of any metal or alloy, and it gave strong evidence of a peculiarity in iron and steel which its magnetic properties alone failed to account for. We could with all non-magnetic metals easily obtain a perfect balance of force by an equivalent piece of the same metal, but in the case of iron, steel, and nickel it was with extreme difficulty that I could obtain a near approach to a perfect zero. Two pieces of iron cut off the same bar or wire, possessing the same magnetic moment, never gave identical results; the difficulty was, that notwithstanding each bar or wire could be easily made to produce the same inductive reaction, the time during which this reaction took place varied in each bar; and although I could easily change its balancing power as regards inductive force by a change in the mass of the metal, by heat or magnetism, the zero obtained was never equal to that obtained from copper or silver.

This led me to suppose the existence of a peculiarity in magnetic metals which could not be accounted for except upon the hypothesis that there was a cause, then unknown, to produce the invariable effect.

Finding that it would be impossible to arrive at the true cause without some new method of investigation, which should allow me to observe the effects from an electrical circuit, whose active portion should be the iron wire itself, I constructed an apparatus or electro-magnetic induction balance, consisting of a single coil reacting upon an iron wire in its axis, and perpendicular to the coil itself; by this means the iron or other wire itself became a primary or secondary, according as the current passed through the coil or wire. Now, with this apparatus we could induce secondary currents upon the wire or coil, if the coil was at any angle, except when the wire was absolutely perpendicular; in this state we could only obtain a current from some disturbing cause, and the current so obtained was no longer secondary but tertiary.

The whole apparatus, however, is more complicated than the general idea given above, as it was requisite not only to produce effects but to be able to appreciate the direction of the electrical current obtained, and have comparative measures of their value. In order to fully understand the mode of experiments, as well as the results obtained, I will first describe the apparatus employed.

The electro-magnetic induction balance consists of—1st, an instrument for producing the new induction current; 2nd, sonometer or balancing coils; 3rd, rheotome and battery; 4th, telephone.

The essential portion of this new balance is that wherein a coil is so arranged that a wire of iron or copper can pass freely through, forming its axis. The iron or copper wire rests upon two supports 20 centims. apart; at one of these the wire is firmly clamped by two binding screws; the opposite end of the wire turns freely on its sup-

port, the wire being 22 centims. long, having 2 centims. projection beyond its support, in order to fasten upon it a key or arm which shall serve as a pointer upon a circle giving the degrees of torsion which the wire receives from turning this pointer. A binding screw allows us to fasten the pointer at any degree, and thus preserve the required stress the time required.

The exterior diameter of the coil is 5½ centims., having an interior vacant circular space of 3½ centims., its width is 2 centims.; upon this is wound 200 metres of No. 32 silk-covered copper wire. This coil is fastened to a small board so arranged that it can be turned through any desired angle in relation to the iron wire which passes through its centre, and it can also be moved to any portion of the 20 centims. of wire, in order that different portions of the same wire may be tested for a similar stress.

The whole of this instrument, as far as possible, should be constructed of wood, in order to avoid, as far as possible, all disturbing inductive influences of the coil upon them.

The iron wire at its fixed end is joined or makes contact with a copper wire, which returns to the front part of the dial under its board and parallel to its coil, thus forming a loop, the free end of the iron wire is joined to one pole of the battery, the copper wire under the board is joined to the rheotome and thence to the battery.

The coil is joined to the telephone; but, as in every instance we can either pass the battery current through the wire, listening to its inductive effects upon the wire, or the reverse of this; I prefer, generally, in order to have no voltaic current passing through the wire, to join the iron wire and its loop direct to the telephone, passing the voltaic current through the coil.

In order to balance, measure, and know the direction of the new induction currents by means of a switching key, the sonometer* I described to the Royal Society is brought into the circuit. The two exterior coils of the sonometer are then in the circuit of the battery, and of the coil upon the board containing the iron wire or stress bridge. The interior or movable coil of the sonometer is then in the circuit of the iron wire and telephone. Instead of the sonometer constructed as described in my paper to the Royal Society, I prefer to use one formed upon a principle I described in "Comptes Rendus," December 30, 1878. This consists of two coils only, one of which is smaller and turns freely in the centre of the outside coil. The exterior coil being stationary, the centre coil turns upon an axle by means of a long (20 centims.) arm or pointer, the point of which moves over a graduated arc or circle. Whenever the axis of the interior coil is perpendicular to the exterior coil, no induction takes place, and we

* "Proc. Roy. Soc.," vol. 29, p. 65.

have a perfect zero: by turning the interior coil through any degree we have a current proportional to this angle, and in the direction in which it is turned. As this instrument obeys all the well-known laws for galvanometers, the readings and evaluations are easy and rapid.

If the coil upon the stress bridge is perpendicular to the iron wire, and if the sonometer coil is at zero, no currents or sounds in the telephone will be perceived, but the slightest current in the iron wire produced by torsion will at once be heard; and by moving the sonometer coil in a direction corresponding to the current, a new zero will be obtained, which will not only balance the force of the new current but indicate its value. A perfect zero, however, will not be obtained with the powerful currents obtained by the torsion of 2 milims. diameter iron wire, we then require special arrangements of the sonometer which are too complicated to describe here.

The rheotome is a clockwork, having a rapid revolving wheel which gives interruptions of currents in fixed cadences in order to have equal intervals of sound and silence. I employ four bichromate cells or eight Daniell's elements, and they are joined through this rheotome to the coil on the stress bridge, as I have already described.

The magnetic properties of iron, steel, nickel, and cobalt, have been so searchingly investigated by ancient as well as by modern scientific authors, that there seems little left to be known as regards its molar magnetism. I use the word molar here simply to distinguish or separate the idea of a magnetic bar of iron or steel magnetised longitudinally or transversely from the polarised molecules which are supposed to produce its external magnetic effects.

Molar magnetism, whilst having the power of inducing an electric current in an adjacent wire, provided that either has motion, or the magnet a change in its magnetic force, as shown by Faraday in 1832, has no power of inducing an electric current upon itself or its own molar constituent, either by motion or change of its magnetic moment. Molecular magnetism (the results of which, I believe, I have been the first to obtain) has no, or a very feeble, power of inducing either magnetism or an electric current in an adjacent wire, but it possesses the remarkable power of strongly reacting upon its own molar wire, inducing (comparatively with its length) powerful electric currents, in a circuit of which this forms a part.

In some cases, as will be shown, we may have both cases existing in the same wire; this occurs when the wire is under the influence of stress, either external or internal; it would have been most difficult to separate these two, as it was in my experiments with the induction balance, without the aid of my new method.

Ampère's theory supposes a molecular magnetism or polarity, and that molar magnetism would be produced when the molecular mag-

netism became symmetrical; and his theory, I believe, is fully capable of explaining the effects I have obtained, if we admit that we can rotate the paths of the polarised molecules by an elastic torsion.

Matteucci made use of an inducing and secondary coil in the year 1847,* by means of which he observed that mechanical strains increased or depressed the magnetism of a bar inside this coil.

Wertheim published in the "Comptes Rendus," 1852,† some results similar to Matteucci; but in the "Annales de Chimie et de Physique," 1857,‡ he published a long series of most remarkable experiments, in which he clearly proves the influence of torsion upon the increment or decrement of a magnetical wire.

Vilari showed§ increase or diminution of magnetism by longitudinal pull according as the magnetising force is less or greater than a certain critical value.

Wiedemann,‖ in his remarkable work "Galvanismus," says that an iron wire through which an electric current is flowing becomes magnetised by twisting the wire. This effect I have repeated, but found the effects very weak, no doubt due to the weak battery I use, viz., four quart bichromate cells.

Sir W. Thomson shows clearly in his remarkable paper "Effects of Stress on the Magnetisation of Iron, Nickel, and Cobalt,"¶ the critical value of the magnetisation of these metals under varying stress, and also explains the longitudinal magnetism produced by Wiedemann as due to the outside molar twist of the wire, making the current pass as in a spiral round a fixed centre. Sir William Thomson also shows clearly the effects of longitudinal as well as transversal strain, both as regards its molar magnetism and its electric conductivity.

My own researches convince me that we have in molecular magnetism a distinct and separate form of magnetism from that when we develop, or render evident, longitudinal or transversal magnetism, which I have before defined as molar.

Molecular magnetism is developed by any slight strain or twist other than longitudinal, and it is only developed by an elastic twist; for, however much we may twist a wire, provided that its fibres are not separated, we shall only have the result due to the reaction of its remaining elasticity.

If we place an iron wire, say 20 centims. long, 1 millim. diameter, in the axis of the coil of the electro-magnetic balance, and if this wire is joined, as described, to the telephone, we find that on passing a electric

* "Compt. Rend.," t. xxiv, p. 301, 1847.
† "Compt. Rend.," t. xxv, p. 702, 1852.
‡ "Ann. de Chim. et de Phys.," (3), t. l, p. 385, 1857.
§ "Poggendorff's Annalen," 1868.
‖ Wiedemann. "Galvanismus," p. 447.
¶ "Phil. Trans.," 1879, p. 55.

current through the inducing coil no current is perceptible upon the iron wire; but if we give a very slight twist to this wire at its free end—one-eighteenth of a turn, or 20°—we at once hear, clear and comparatively loud, the currents passing the coil; and although we only gave a slight elastic twist of 20° of a whole turn, and this spread over 20 centims. in length, making an extremely slight molar spiral, yet the effects are more powerful than if, using a wire free from stress, we turned the whole coil 40°. The current obtained when we turn the coil, as just mentioned, is secondary, and with the coil at any angle any current produced by its action, either on a copper, silver, iron, or steel wire; in fact, it is simply Faraday's discovery; but the current from an elastic twist is no longer secondary under the same conditions, but tertiary, as I shall demonstrate later on. The current passing through the coil cannot induce a current upon a wire perpendicular to itself, but the molecules of the outside of the wire, being under a greater elastic stress than the wire itself, they are no longer perpendicular to the centre of the wire, and consequently they react upon this wire as separate magnets would upon an adjacent wire. It might here be readily supposed that a wire having several twists, or a fixed molar twist of a given amount, would produce similar effects. It, however, does not, for in most cases the current obtained from the molar twists are in a contrary direction to that of the elastic torsion. Thus, if I place an iron wire under a right-handed elastic twist of 20°, I find a positive current of 50° sonometer; but if I continue this twist so that the index makes one or several entire revolutions, thus giving a permanent molar twist of several turns, I find upon leaving the index free from any elastic torsion, that I have a permanent current of 10°, but it is no longer positive, but negative, requiring that we should give an elastic torsion in the previous direction, in order to produce a positive current. Here a permanent elastic torsion of the molecules is set up in the contrary direction to its molar twist, and we have a negative current, overpowering any positive current which should have been due to the twisted wire.

The following table shows the influence of a permanent twist, and that the current obtained when the wire was freed from its elastic torsion was in opposition to that which should have been produced by the permanent twist. Thus, a well-softened iron wire, 1 millim. in diameter, giving 60° positive current for a right-handed elastic torsion of 20°, gave after 1°·80 permanent torsion a negative current of 10°.

1 complete permanent torsion	(right-handed)	negative	....	10
2 ,,	,,	,,	....	15
3 ,,	,,	,,	....	15
4 ,,	,,	,,	....	16
5 ,,	,,	,,	...	12

6 complete	permanent	torsion (right-handed)	negative	10
7	"	"	"	5
8	"	"	"	4
9	"	"	"	3
10	"	"	"	3

At this point the fibres of a soft wire commence to separate, and we have no longer a complete single wire, but a helix of separate wires upon a central structure.

If now, instead of passing the current through the coil, I pass it through the wire, and place the telephone upon the coil circuit, I find that I obtain equally as strong tertiary currents upon the coil as in the previous case, although in the first case there was produced longitudinal electro-magnetism in the perpendicular wire by the action of the coil, but in the latter case none or the most feeble electro-magnetism was produced, yet in these two distinct cases we have a powerful current produced not only upon its own wire, but upon the coil, thus proving that the effects are equally produced both on the wire and coil.

If we desire, however, in these reversible effects to produce in both cases the same electromotive force, we must remember that the tertiary current when reacting upon its own short wire produces a current of great quantity, the coil one of comparative higher intensity. We can, however, easily convert the great quantity of the wire into one of higher tension by passing it through the primary of a small induction coil whose resistance is not greater than one ohm. We can then join our telephone, which may be then one of a high resistance, to the secondary of this induction coil, and by this means, and without changing the resistance of the telephone, receive the same amount of force, either from the iron wire or the coil.

Finding that iron, steel, and all magnetic metals produce a current by a slight twist, if now we replace this wire by one of copper or non-magnetic metals we have no current whatever by an elastic twist, and no effects, except when the wire itself is twisted spirally in helix, and whatever current we may obtain from copper, &c., no matter if from its being in spiral or from not being perpendicular to the axis of the coils, the currents obtained will be invariably secondary and not tertiary. If we replace the copper by an iron wire, and give it a certain fixed torsion, not passing its limit of elasticity, we find that no increase or decrease takes place by long action or time of being under strain. Thus a wire which gave a sonometric force of 50° at the first observation, remained perfectly constant for several days until it was again brought to zero by taking off the strain it had received. Thus we may consider that as long as the wire preserves its elasticity, exactly in the same ratio will it preserve the molecular character of its magnetism.

It is not necessary to use a wire to produce these effects; still more powerful currents are generated in bars, ribbons, or sheets of iron; thus, no matter what external form the iron may possess, it still produces all the effects I have described.

It requires a great many permanent twists in a wire to be able to see any effect from these twists, but if we give to a wire, 1 millim. diameter, forty whole turns (or until its fibres become separated) we find some new effects; we find a small current of 10° in the same direction as its molar twist, and on giving a slight twist (20°) the sonometric value of the sound obtained is 80°, instead of 50°, the real value of a similar untwisted wire; but the explanation will be found by twisting the wire in a contrary direction to its molar twist. We can now approach the zero but never produce a current in the contrary direction, owing to the fact that by the spiral direction, due to the fibrous molar turns, the neutral position of its molecules is no longer parallel with its wire, but parallel with its molar twist, consequently an elastic strain in the latter case can only bring the molecules parallel with its wire, producing no current, and in the first case the angle at which the reaction takes place is greater than before, consequently the increased value of its current.

The measurements of electric force mentioned in this paper are all sonometric on an arbitrary scale. Their absolute value has not yet been obtained, as we do not, at our present stage, require any except comparative measures.* Thus, if each wire is of 1 millim. diameter and 20 centims. long, all render the same stress in the axis of its coil. I find that the following are the sonometric degrees of value:—

	°	
Soft iron	60	Tertiary current.
Hard drum iron	50	,, ,,
Soft steel	45	,, ,,
Hard tempered steel	10	,, ,,
Copper, silver, &c.	—0	
Copper helix, 1 centim. diameter, 20 turns in 20 centims.	45	Secondary currents.
Iron, spiral, ditto	45	,, ,,
Steel	45	,, ,,

The tertiary current increases with the diameter of the wire, in a proportion which has not yet been determined; thus, an ordinary hard iron wire of 1 millim. diameter giving 50°, one of 2 millims. diameter gave 100°; and the maximum of force obtained by any degree of torsion is at or near the limit of elasticity, since as soon as we pass this point, producing a permanent twist, the current decreases, as I have already shown in the case of a permanent twist. Thus, the

* 50° sonometer has the same electromotive force as 0·10 of a Daniell battery.

critical point of 1 millim. hard iron wire was 20° of torsion, but in hard steel it was 45°.

Longitudinal strains do not produce any current whatever, but a very slight twist to a wire, under a longitudinal strain, produces its maximum effects: thus, 20° of torsion being the critical point of iron wire, the same wire, under longitudinal strain, required but from 10° to 15°. It is very difficult, however, to produce a perfect longitudinal strain alone. I have, therefore, only been able to try the effect of longitudinal strain on fine wires, not larger than 1 millim. in diameter, but as in all cases, no effect whatever was produced by longitudinal strain alone, I believe none will be found if the wire be absolutely free from torsion. The molecules in a longitudinal strain are equally under an elastic strain as in torsion, but the path of their motion is now parallel with its wire, or the zero of electric inductive effect, but the longitudinal and transverse strains of which the compound strain is composed, react upon each other, producing the increased effect due to the compound strain.

The sonometer is not only useful for showing the direction of the current and measuring it by the zero method, but it also shows at once if the current measured is secondary or tertiary. If the current is secondary its period of action coincides with that of the sonometer, and a perfect balance, or zero of sound, is at once obtained, and its value in sonometric degrees given, but if the current is tertiary, no zero is possible, and if the value of the tertiary is 60°, we find 60° the nearest approach to zero possible. But by the aid of separate induction coils to convert the secondary into a tertiary, a perfect zero can be obtained if the time of action and its force correspond to that which we wish to measure.

If I place a copper wire in the balance and turn the coils at an angle of 45°, I obtain a current for which the sonometer gives a perfect zero at 50°, proving, as already said, that it is secondary. If I now replace the copper by an iron wire, the coil remaining at 45°, I have again exactly the same value for the iron as copper, viz., 50°, and in both cases secondary. Now, it is evident that in the case of the iron wire there was produced at each passage of the current a strong electro-magnet, but this longitudinal magnetism did not either change the character of the current or its value in force.

A most beautiful demonstration of the fact that longitudinal magnetism produces no current, but that molecular magnetism can act equally as well, no matter the direction of the longitudinal magnetism, consists in forming an iron wire in a loop, or taking two parallel but separate wires, joined electrically at their fixed ends, the free ends being each connected with the circuit, so that the current generated must pass up one wire and down the adjacent one. On testing this loop, and if there are no internal strains, complete silence or absence

of current will be found. Now, giving a slight torsion to one of these wires in a given direction, we find, say, 50° positive; twisting the parallel wire in a similar direction produces a perfect zero, thus, the current of the second must have balanced the positive of the first. If, instead of twisting it in similar directions, we twist it in the contrary direction, the sounds are increased in value from 50° positive to 100° positive, showing, in this latter case, not only a twofold increase of force, but that the currents in the iron wires travelled up one wire and down the other, notwithstanding that both were strongly magnetic by the influence of the coil in one direction, and this experiment also proves that its molar magnetism had no effect, as the currents are equally strong in both directions, and both wires can double or efface the currents produced in each. If, instead of two wires we take four, we can produce a zero, or a current of 200°, and with twenty wires we have a force of 1,000°, or an electromotive force of two volts. We have here a means of multiplying the effects by giving an elastic torsion to each separate wire, and joining them electrically in tension. If loops are formed of one iron and one copper wire, we can obtain both currents from the iron wire, positive and negative, but none from the copper, its *rôle* is simply that of a conductor upon which torsion has no effect.

I have already mentioned that internal strains will give out tertiary currents, without any external elastic strain being put on. In the case of iron wire, these disappear by a few twists in both directions, but in flat bars or forged iron, they are more permanent; evidently, portions of these bars have an elastic strain, whilst other portions are free, for I find a difference at every inch tested: the instrument, however, is so admirably sensitive, and able to point out not only the strain, but its direction, that I have no doubt its application to large forged pieces, such as shafts or cannon, would bring out most interesting results, besides its practical utility; great care is therefore necessary in these experiments that we have a wire free from internal strains, or that we know their value.

Magnetising the iron wire by a large steel permanent magnet has no effect whatever. A hard steel wire thus placed becomes strongly magnetic, but no current is generated, nor has it any influence upon the results obtained from molecular movement, as in elastic torsion. A flat wide iron or steel bar shows this better than iron wire, as we can here produce transversal, instead of longitudinal, but neither shows any trace of the currents produced by molecular magnetism. I have made many experiments with wires and bars thus magnetised, but as the effect in every case was negative when freed from experimental errors, I will not mention them; but there is one very interesting proof which the instrument gives, that longitudinal magnetism first passes through its molecular condition before and during the discharge,

or recomposition of its magnetism. For this purpose, using no battery, I join the rheotome and telephone to the coil, the wire having no exterior circuit. If I strongly magnetise the two ends of the wire, I find by rapidly moving the coil, that there is a Faradaic induction of 50° at both poles, but very little or none at the centre of the wire; now fixing the coil at the centre or neutral point of the wire, and listening intently, no sounds are heard, but the instant I give a slight elastic torsion to the free pole, a rush of electric tertiary induction is heard, whose value is 40°. Again, testing this wire by moving the coil, I find only a remaining magnetism of 10, and upon repeating the experiment of elastic torsion, I find a tertiary of 5; thus we can go on gradually discharging the wire, but it will be found that its discharge is a recomposition, and that it first passed through the stage I have mentioned.

Heat has a very great effect upon molecular magnetic effects. On iron it increases the current, but in steel the current is diminished. For experimenting on iron wire, which gave a tertiary current of 50° positive (with a torsion of 20°), upon the application of the flame of a spirit-lamp, the force rapidly increases (care being taken not to approach red heat), until the force is doubled, or 100 positive. The same effects were obtained in either direction, and were not due to a molar twist or thermo-current, as if care had been taken to put on not more than 10° of torsion, the wire came back to zero at once on removal of the torsion. Hard tempered steel, whose value was 10° whilst cold, with a torsion of 45°, became only 1° when heated, but returned (if not too much heated) to 8° when cold. I very much doubted this experiment at first, but on repeating the experiment with steel several times, I found that on heating it, I had softened the extreme hard (yellow) temper to that of the well-known blue temper. Now, at blue temper, hot, the value of steel was but 1° to 2°, whilst soft iron of a similar size gave 50° of force cold, and 100° at red heat. Now, as I have already shown that the effects I have described depend on molecular elasticity, it proves at least, as far as iron and steel are concerned, that a comparatively perfect elastic body, such as tempered steel, has but slight molecular elasticity, and that heat reduces it, but that soft iron, having but little molar elasticity, has a molecular elasticity of a very high degree, which is increased by heat.

The objects of the present paper being to bring the experimental facts before the notice of the Royal Society, and not to give a theoretical solution of the phenomena, I will simply add, that if we assume with Poisson, that the paths of the molecules of iron are circles, and that they become ellipses by compression or strain, and also that they are capable of being polarised, it would sufficiently explain the new effects.

Joule has shown that an iron bar is longer and narrower during magnetisation than before, and in the case of the transverse strain, the exterior portions of the wire are under a far greater strain than those

near the centre, and as the polarised ellipses are at an angle with the molecules of the central portions of the wire, its polarisation reacts upon them, producing the comparatively strong electric currents I have described.

III. "On the Action of Sodium upon Chinoline." By C. Greville Williams, F.R.S. Received March 8, 1881.

In 1867 I made some experiments on the action of sodium upon chinoline and lepidine, and found that a substance was produced which dyed silk a beautiful but fugitive orange colour. I announced this fact in a paper "On the Higher Homologues of Chinoline."* I made analyses of the products at the time, but the difficulties in the way of preparing them pure were so great, and the time at my disposal was so limited, that I did not make public any quantitative results until March, 1878, when I published a short note "On the Action of Sodium on Chinoline and Lepidine."† In that paper I gave the results of an analysis of a product from chinoline, which agreed with the formula $C^{18}H^{14}N^{2}.HCl$, which is obviously that of the hydrochlorate of dichinoline. I also gave an analysis of the nitrate of dilepidine, which agrees almost perfectly with the theoretical values; but I did not enter into the details of the modes of preparation. In the course of my investigation of β-lutidine, it was natural that I should study the action of sodium upon it, but I met with so many and unexpected difficulties that I determined to prepare myself for a new attack upon the subject by a fresh investigation of the action of sodium upon chinoline. As I find that other observers are working upon chinoline, β-lutidine, and β-collidine,‡ I have thought it desirable to bring before the Society the results obtained, although the investigation is still proceeding.

Action of Sodium upon Chinoline.

The action of sodium on chinoline is exceedingly remarkable, not merely because it polymerises the base, for a similar result, as is well known, takes place with picoline, but because the products have properties which are, I think, different from any yet observed among organic substances. For a yellow oil like dichinoline to yield a true although fugitive dye, in the form of a brilliantly red crystalline

* "Laboratory," May, 1867, p. 109.

† "Chemical News," March 1, 1878.

‡ Richard, "Bull. Soc. Chim.," [2] xxxii, 486—489; Boutlerow and Wischnegradsky, *loc. cit.*, No. 9, June 5, 1880; Oechsner de Coninck, "Bull. Soc. Chim.," Nos. 4 and 5, p. 210, September 5, 1880. The latter chemist has repeated several of my older experiments, evidently under the impression that they had not been made before.

hydrochlorate, is probably a unique reaction. To obtain this product of the utmost beauty, it appears to be necessary that the chinoline should be perfectly free from any impurity, except, perhaps, its next homologue. The most successful preparations were made from chinoline obtained from fine crystals of the chromate.*

Chinoline prepared from the chromate is almost colourless, and becomes yellow, on keeping, with extreme slowness as compared with the base prepared without that precaution. In the following experiments it is to be understood that all the chinoline was obtained from the crystallised chromate.

Chinoline was boiled with sodium, the fluid, which became purplish crimson, was treated with water, which at once converted the crimson colour into a dirty yellow. Hydrochloric acid was then added, and the solution became of an intense orange colour. The solution was boiled, filtered, and allowed to stand two days, the crystals of hydrochlorate of dichinoline were filtered off, and the mother-liquid was precipitated fractionally by solution of platinic chloride. The first precipitate was of a light orange colour, the second a deep orange, the third Naples yellow, the fourth was in the form of a brown crystalline powder. The platinum was determined in each with the following results:—

No. of precipitate.	Percentage of platinum.
I	22·59
II	23·83
III	28·30
IV	28·59

The first result agrees with the numbers required for the formula—

$$2(C^{18}H^{14}N^{2})HCl.PtCl^{4},$$

which requires 22·18 per cent. of platinum. The second is probably a mixture of the substance having the formula of the first precipitate with some of that having the formula of the third, which latter agrees fairly with a salt having the formula—

$$C^{18}H^{14}N^{2}.3HCl.PtCl^{4},$$

which requires 28·00 per cent. of platinum. The formula—

$$C^{18}H^{14}N^{2}.2HCl.PtCl^{4},$$

requires 29·51 per cent. There are other formulæ which agree with

* It is much to be desired that those chemists who believe in the identity of chinoline and leucoline, and who possess the latter in a pure state, would study the action of sodium upon it; as, if it yields the crystalline scarlet hydrochlorate, the question might be considered as settled.

the numbers obtained, but they do not appear to me so readily admissible. For instance, a substance having the formula—

$$2(C^{18}H^{14}N^{2})PtCl^{4},$$

would require 23·13 per cent. of platinum, which would agree with the amount of metal obtained from the second precipitate, but the formation of such a substance in the presence of free hydrochloric acid appears most unlikely. Nevertheless, it is remarkable that the platinum salts obtained from the products of the action of sodium on chinoline and β-lutidine, often agree in their percentages of platinum with substances having formulæ of the polymerised bases united directly to platinic chloride.

Action of Sodium Amalgam on Chinoline.

25 grms. of chinoline and 20 grms. of sodium amalgam containing 10 per cent. of sodium were mixed. The action commenced in the cold, and red streaks appeared where the base and the amalgam were in contact. When the action ceased in the cold, the mixture was warmed on the water-bath; the reaction started again immediately, and the whole became of a deep red colour. No gas was given off. The next day the mixture had assumed the consistence of thick treacle; when heated on the water-bath it softened, but the amalgam remained hard; the fluid had turned from purplish red to yellow, but reddish streaks were still formed on stirring, showing that the reaction was not complete. At this stage 5 grms. more amalgam were added. After four or five hours of constant stirring on the water-bath the red streaks ceased to appear, and the whole became a greenish yellow oil, the greater portion of which was poured off, and the residue containing much undecomposed amalgam was treated with hot water. The amalgam decomposed with violence, and when the action was over excess of hydrochloric acid was added, and the solution was boiled and filtered. Three liquors were taken off and kept separate from each other, each solution gave on cooling crystals of the hydrochlorate of dichinoline; they were in the form of needles of so brilliant a scarlet that, on comparison with a sample of the finest vermillion, the latter looked brown by contrast with them. The first solution yielded 1·1 grams of air-dried product, the second 0·4, and the third 0·25, in all 1·75 grms. from 25 grms. of chinoline. The scarlet crystals bleach on exposure to light and become pale brown on drying in the water-oven, and the brown colour itself rapidly bleaches on exposure to light.

The mother-liquors of the scarlet crystals on evaporation to a quarter of their original bulk, deposited a pale yellow precipitate too small for analysis; it was nearly insoluble in cold water.

The mother-liquors affording no further crystals on evaporation, were treated with excess of potash to enable the chinoline to be

recovered; 11 grms. were obtained, being 44 per cent. on the chinoline taken; deducting the recovered chinoline from that originally employed, the produce of the scarlet crystals become 12·5 per cent. on the base originally taken.

Action of Sodium Amalgam on the Recovered Chinoline.

The recovered chinoline was dried by sticks of potash for a short time, and then rectified to see if its boiling-point had undergone any alteration by the treatment with sodium amalgam; however, it came over at almost exactly the same point as it did before. It is very remarkable that in spite of the fact last mentioned, the product of the action of the amalgam upon the recovered base was of a totally different character to that of the same reagent upon the original chinoline.

The 11 grms. of recovered chinoline were treated with 20 grms. of amalgam, effervescence took place, and much heat was developed; this was possibly caused by traces of moisture remaining in the base. When the effervescence was over, 5 grms. more amalgam were added, being in all 30 grms. for 11 of base, whereas in the first experiment equal parts of base and amalgam were used; this fact has to be remembered in considering the causes of the different result. The red coloration took place as before; it is, however, a very ephemeral reaction, the colour on a glass rod soon disappears, and does so immediately on spreading the substance in a thin layer on the side of the beaker in which the operation is conducted. The experiment was made precisely as with the original chinoline, and the reaction appeared to proceed exactly in the same manner as before; but, on boiling out with dilute hydrochloric acid, the product of hydrochlorate of dichinoline was much smaller, and the crystals were too small to be distinguished as such by the naked eye.

The mother-liquors were treated with potash, as before, to enable the chinoline to be recovered, when to my surprise a *solid yellow base* was obtained, which on boiling some time with water to expel any adhering chinoline, became, on cooling, a hard resinous mass: on drying in the air it weighed 7 grms., or 63 per cent. on the 11 grms. of recovered chinoline used.

In order to gain some insight into the nature of the new solid base, it was converted into a hydrochlorate and fractionally precipitated with platinic chloride. 1 grm. was dissolved in 50 cub. centims. of hot diluted hydrochloric acid, on cooling a part separated out; this could have been prevented by a great excess of hydrochloric acid, but, on this occasion, the separation was permitted as a mode of purification. The precipitate was filtered off, and some hydrochloric acid was added to prevent the water in the solution of platinic chloride from causing a further precipitation. On adding the pla-

tinum solution, a dense bright yellow precipitate was formed; to the filtrate more platinum solution was added, and so on until four precipitates were obtained. The second, third, and fourth were buff coloured, but the last on standing became of a dirty brown colour. The two last were in too small quantity for analysis.

The first precipitate gave 18·47 per cent. of platinum. Assuming for the present that the yellow solid base from the recovered chinoline is isomeric with that which yields the scarlet hydrochlorate, it may be mentioned that a salt having the formula—

$$2\,(C^{18}H^{14}N^2)4HCl.PtCl^4$$

would require 19·76 per cent. of platinum. The second precipitate gave 23·49 per cent. of platinum, and yielded, therefore, almost the same percentage of the metal as the second precipitate obtained from the product of the action of sodium upon the original chinoline, and the remarks made upon that precipitate apply equally to the present one.

Experiments were subsequently made to ascertain if, in presence of excess of hydrochloric acid, platinum salts of a different constitution would be formed. For this purpose 1 grm. of the new solid base was treated with 10 cub. centims. of *strong* hydrochloric acid, it dissolved with effervescence as if carbonic anhydride had been absorbed; the solution took place with moderate readiness; it was filtered and treated with solution of platinum as before, a buff coloured precipitate was obtained; at this point 10 cub. centims. more hydrochloric acid were added, the precipitate became smaller, 25 cub. centims. more hydrochloric acid were added at this stage, and the solution was heated to about 90° and filtered; much of the platinum salt remained on the filter, and, after drying, gave 23·92 per cent. of platinum, the mother-liquid on cooling gave another precipitate containing 24·55 per cent. of platinum; both these precipitates appear, therefore, in spite of the great excess of acid in which they were formed, to be constituted like the second precipitates previously obtained.

The solid yellow base contained 3·06 per cent. of ash, which consists of carbonate of lime with a trace of iron, both derived from the chemicals used in its preparation.

The solid base when heated fuses to a yellow oil, giving off precisely the same odour as chinoline compounds generally; near a red heat it boils away leaving some carbon which, contrary to the usual rule in such cases, burns away readily.

Nitric acid dissolves the base easily, giving a reddish brown solution which on dilution with water affords a yellow precipitate; ammonia turns the solution red, and partially dissolves the precipitate.

Preparation of the Platinum Salt from the Scarlet Hydrochlorate of Dichinoline.

The scarlet hydrochlorate of dichinoline was dissolved in boiling water, and precipitated while boiling by an excess of platinic chloride. The high temperature of the solution was necessary to prevent the precipitation of the hydrochlorate with the platinum salt. The latter was of a beautiful pale cadmium yellow colour. The filtrate on cooling yielded fine needles in extremely small quantity. The precipitate contained 24·54 per cent. of platinum, and probably, therefore, had the same composition as those before mentioned, containing the same or nearly the same amount of platinum. The needles contained 35·7 per cent. of platinum, and it is most likely that the presence of a small quantity of this salt was the cause of the high platinum in the first precipitate. A direct combination of dichinoline with platinic chloride—

$$C^{18}H^{14}N^{2}.PtCl^{4},$$

would require 33·11 per cent. of platinum, but it is not easy, as I have said before, to admit such a formula, when it is remembered that the precipitation took place in the presence of hydrochloric acid.

A salt of the formula—

$$2(C^{18}H^{14}N^{2})HCl.2PtCl^{4},$$

would require 33·66 per cent. of platinum, and would only differ from the salt—

$$2(C^{18}H^{14}N^{2})HCl.PtCl^{4},$$

before alluded to, and which contains 22·18 per cent. of platinum, by containing one more equivalent of platinic chloride. It must, however, be distinctly stated that the numbers in this paper are given provisionally, and that the subject is still under study.

Presents, March 3, 1881.

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END OF THE THIRTY-FIRST VOLUME.

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Society, a Fellow of the Institute of Chemistry, an honorary member of the Berlin Chemical Society, as also of the Philosophical Society of Manchester, and the Pharmaceutical Society of Great Britain.

It will be evident from an inspection of the titles of the numerous papers (more than 100 in number) published by Dr. Stenhouse during the past forty years, in the Transactions and Proceedings of the Royal Society, the "Journal of the Chemical Society," "Liebig's Annalen," and other scientific journals (either alone or in conjunction with Mr. C. E. Groves), that these for the most part relate to what may truly be called "Organic Chemistry," the chemistry of compounds found in organised bodies, so that his name will long be associated with numerous carbon compounds obtained from plants, and derivatives formed from them. Among all these he applied himself chiefly to the principles from the lichens, and made known the results in eighteen papers. One of his communications, published in 1880, is worth mention as it relates to "Betorcinol," a substance he had discovered some thirty-two years previously. It is but seldom that a chemist lives to complete a work begun so long before.

Although the eminence he attained in organic research is fully recognised, his contributions to our technical knowledge are not so generally known. He was the author of many ingenious and useful inventions in dyeing, waterproofing, sugar manufacture, and tanning; but the greatest and most permanent benefit has been conferred by his application of the powerful absorbent properties of wood charcoal to disinfecting and deodorising purposes, which took the form of charcoal air-filters and charcoal respirators.

Of Dr. Stenhouse's personal character, those who knew him intimately could never speak too highly, his general conversation and fund of anecdote rendering him a most pleasant companion. His ingenuity and quick perception were remarkable, and this combined with his unflagging industry, and patience and resignation in great bodily suffering, enabled him to continue his scientific work with unabated vigour, even after the effects of paralysis prevented him from performing experiments with his own hands.

Humphrey Lloyd was born in Dublin on the 16th of April, 1800. His father was the Rev. Bartholomew Lloyd, afterwards Provost of Trinity College, Dublin, at that time a Junior Fellow. Having received his early education at Mr. White's school in Dublin, he entered Trinity College on July 3, 1815, obtaining at the Entrance Examination, which was at that time altogether classical, first place among sixty-three competitors. He obtained Scholarship (Classical) in 1818. At the examination for the degree of B.A. he obtained the Science Gold Medal, the highest honour which could be gained by an undergraduate. In the year 1824 he obtained a Fellowship, given then, as

now, to the best answerer at a special examination. He was elected, in 1831, to the chair of Natural and Experimental Philosophy, which he filled with distinguished ability till 1843, when he became a Senior Fellow. In the year 1862 he became Vice-Provost, and in 1867, when the Provostship became vacant by the death of Dr. Macdonnell, he was chosen by the Government of the day to fill the place. He continued to discharge the duties of this important office with unwearied assiduity till his death, which occurred, after a few days' illness, on the 17th of January, 1881.

Dr. Lloyd was President of the Royal Irish Academy from 1846 to 1851, and on the visit of the British Association to Dublin, in 1857, he was elected to the Presidency of that distinguished Society.

In 1856 the University of Oxford conferred on him the degree of D.C.L., *Honoris Causa*, and in 1874 he received from the Imperial Government of Germany the Cross of the Order "Pour le Mérite."

Dr. Lloyd's most important contributions to science were made in the departments of optics and magnetism. It will be convenient to consider these subjects separately, taking the contributions to each subject respectively in the order of time.

His first contribution to optical science was a systematic work on plane (as distinguished from physical) optics. It was entitled "A Treatise on Light and Vision," and was published in the year 1831. This book possesses a high scientific value.

The year 1832 was distinguished in Dr. Lloyd's life by, perhaps, his most remarkable single scientific achievement, namely, the experimental proof of the phenomenon of conical refraction. The discovery of conical refraction presents one of the instances—rare in the history of physical science—in which theory was able not merely to account for a phenomenon but to predict it. Reasoning mathematically on the theory of Fresnel, and giving a suitable physical interpretation to the mathematical results which he obtained, Professor (afterwards Sir William) Hamilton deduced the remarkable consequence that, in certain cases, the two rays into which an incident ray is usually divided by a crystal are replaced by an infinite number of rays, forming a luminous cone or cylinder. Anxious to submit this extraordinary result to the test of experiment, he requested Dr. Lloyd to undertake the experimental investigation of the phenomenon. It would be impossible to give here a detailed account of the difficulties attendant upon this inquiry. Suffice it to say that they were overcome by the experimental ability of Dr. Lloyd, who succeeded in giving a perfect experimental demonstration of this remarkable phenomenon in both its varieties. He also established experimentally the law by which the polarisation of the rays composing the luminous cone is governed.

This successful investigation at once brought Dr. Lloyd to the front

rank among the cultivators of optical science, and in the year 1833 he was requested by the British Association to report on the condition of physical optics. The report prepared in compliance with this request was laid before the British Association in the year 1834, and may be regarded as a handbook of the progress of the science to that date.

Shortly after the publication of the experiment which established the reality of conical refraction, Dr. Lloyd described to the Royal Irish Academy an important experiment upon the interference of light proceeding directly from a luminous source with light coming from the same source, but reflected at a very high angle of incidence from a plane surface. By means of this experiment he was able to make an important contribution to the theory of reflected light. The phenomena of thin plates require us to admit that a semi-undulation is gained or lost by the light in the process of reflexion at one of the surfaces. But these phenomena do not decide the question whether this modification takes place at the surface of the rarer or of the denser medium. Dr. Young had given the preference to the former of these alternatives; but Dr. Lloyd derived from the above-mentioned experiment a strong argument in favour of the other. The details of this experiment are published in the seventeenth volume of the "Transactions of the Royal Irish Academy."

In 1836 Dr. Lloyd published the first part of his lectures on the "Wave Theory of Light," including the phenomena which are independent of polarisation and double refraction. To this was subsequently added a second part in which the phenomena of polarisation are discussed.

A communication received from Sir David Brewster, detailing some remarkable appearances which he had observed in connexion with the phenomena of thin plates, induced Dr. Lloyd to turn his attention to that subject, the light incident on the plate being supposed to be polarised. A communication on this subject was made by him to the British Association in 1841, but the complete investigation of the phenomenon was published in the twenty-fourth volume of the "Transactions of the Royal Irish Academy," having been laid before that Society in 1859.

Assuming the truth of Fresnel's expressions for the intensity and phase of polarised light reflected from the surface of an ordinary medium, Dr. Lloyd showed that the reflected light is elliptically polarised. He assigned the law of this elliptic polarisation, which passes into plane polarisation where the incident light is polarised in, or at right angles to, the plane of incidence. He also gave the explanation of the phenomena observed by Brewster, where the index of refraction of the plate is intermediate between those of the bounding media.

But it will probably be felt that the link which associates Dr. Lloyd's

name most indissolubly with the science of the nineteenth century is to be found in the subject of terrestrial magnetism. It is here that his labours, whether conducted singly or in association with other investigators, have left the most permanent mark; and it is not too much to say that no single individual contributed more largely to the success of the effort which was made to perfect by observation our knowledge of the earth's magnetic force.

At the first meeting of the British Association in 1831, the Committee of Section A reported that it was highly desirable that a series of observations upon the intensity of terrestrial magnetism in various parts of England be made by some competent individual, similar to those which had been recently carried on in Scotland by Mr. Dunlop. In compliance with this suggestion, some experiments were made in the neighbourhood of Liverpool, by W. S. Traill, M.D., the results of which were laid before the Association in 1832. In 1833 the Committee extended their recommendation so as to include the whole kingdom, appointing as a Standing Committee, charged with the promotion of these objects, Professors Christie, Forbes, and Lloyd. At the same time Dr. Lloyd undertook to make the required observations in Ireland. These observations were carried on in the year 1834 by Dr. Lloyd, with the assistance of Captain (afterwards Sir Edward) Sabine, and subsequently of Captain (afterwards Sir James) Ross. A brief communication on the method of these observations was made by Dr. Lloyd to the Association in 1834. In this communication he described a new method of obtaining the values of the intensity and the dip, by observation of a magnet turning round a horizontal axis. This method had been previously communicated to the Royal Irish Academy by Dr. Lloyd (October, 1833) and is published in the seventeenth volume of their "Transactions." A full report of observations made at twenty-four stations in various parts of Ireland was laid before the British Association in 1835, and is published, along with a magnetic chart of Ireland, in the Report for that year. This was followed, in 1838, by an elaborate memoir, "On the Magnetic Survey of Great Britain," from the pen of Major Sabine, embodying the results of observations made by several distinguished physicists, including Dr. Lloyd.

In the same year the Association came to the conclusion that a wider scope ought to be given to the inquiry, by establishing stations for *simultaneous* magnetic observations in various parts of the world. As it was impossible for private individuals to carry out so extensive a plan, it was resolved to endeavour to obtain the co-operation of the Government, and a Committee, consisting of Sir J. Herschel, Dr. Whewell, Mr. Peacock, and Dr. Lloyd, was appointed for this purpose. The representations made by this Committee, strongly backed by the Royal Society, were successful, and Magnetic Observa-

tories were erected in various parts of the world. The high appreciation formed of Dr. Lloyd's scientific ability was proved by the fact that the duty of drawing up instructions for the observers at these several stations was intrusted to him. It was also provided that he should receive directly from the stations the reports of the observers. As a further proof of Dr. Lloyd's high reputation in this branch of science, it may be mentioned that the Director of the Magnetic Observatory at Cadiz visited Dublin for the purpose of consulting him as to the best methods of observing.

In the year 1837, Dr. Lloyd had induced the Board of Trinity College to commence the erection of a Magnetic Observatory within the precincts of the College. This observatory was completed in the year 1838, and observations were regularly carried on under his personal superintendence for several years. The instruments used in this observatory were constructed under the direction of Dr. Lloyd, and were in large part devised or greatly modified by him. Instruments similar in their construction to those used in the Dublin Observatory were subsequently employed in other observatories throughout the world. The observations made in Dublin were published in two quarto volumes in the year 1869. They form an important part of the great series of magnetic observations begun by an individual in Scotland—passing thence into the hands of a Society, and by them extended throughout the British Isles—taken up finally by the nation and extended over the world. No nation ever engaged in a more important scientific enterprise; no individual held a more prominent place in that enterprise than Dr. Lloyd.

Besides his share in this great public work, Dr. Lloyd was continually and actively engaged in his individual researches, the results of which were from time to time given to the world, generally in the "Transactions" or "Proceedings" of the Royal Irish Academy. Among these may be noticed a memoir "On the Mutual Action of Two Permanent Magnets," readbefore the Academy in February, 1839. The importance of this communication in its bearing on the question of terrestrial magnetism, was recognised by the Committee in their "Report to the British Association," in 1841. Besides optics and magnetism, Dr. Lloyd communicated to the Royal Irish Academy, from time to time, valuable papers on meteorology. In the year 1877, he published in a collected form, under the title "Miscellaneous Papers connected with Physical Science," his principal contributions to the Transactions of the British Association, and of the Royal Irish Academy. He also published in 1874, a systematic treatise on "Magnetism, General and Terrestrial." The third edition of his treatise on the "Wave Theory of Light" was published in 1873.

As head of the University of Dublin, Dr. Lloyd won golden opinions from all those who came into relation with him. His policy

was pure and liberal, guided by broad principles, and ever mindful of the greatness of the institution which he ruled. His University—his country—the world of science, will remember him long.

J. H. J.

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